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IN THIS ISSUE

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Erratum

In the Research Note "Sex Differences and Psychiatric Disorders" (vol. 81, May 1976), on page 1450, the median rate of psychosis for males in studies published in 1950 or later should be 1.20 instead of .120 in table 2.

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IN THIS ISSUE

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Some Real and Imaginary Consequences of Social Mobility: A French-American Comparison[1]

Melvin Seeman
University of California, Los Angeles

The consequences (correlates) of intergenerational occupational mobility were examined among French and American male workers. The comparative interest focused on two features: (1) the presumed sharper discontinuity of French class structure compared with the more diffuse American system; and (2) the supposed differences in mobility attitudes in the two countries. Structure and attitudes are commonly taken as conditioners of the mobility experience, moderating or heightening its hypothesized effects on (for example) feelings of social isolation, ethnic prejudice, sense of relative deprivation, and political radicalism or conservatism The analysis, however, argues strongly in support of French-American similarity rather than difference, in two major respects: (1) mobility attitudes showed the same basic structure and correlation pattern; and (2) as expected, the effects which can be attributed to objective mobility were highly limited in both countries.

Many sociologists are convinced that both downward and upward occupational mobility have important personal and social consequences (e.g., high prejudice, uniquely conservative or leftist politics, social isolation, etc.), but the evidence to sustain this conviction about social mobility is unimpressive.

Still, sociologists seem reluctant to accept the disconfirmation of a favorite hypothesis. Thus, to give one example, Silberstein and Seeman (1959) showed long ago that occupational mobility in itself was not predictive of anti-Semitism or anti-Negro prejudice, a negative result that has been sustained in analyses based on other methods (e.g., dummy-variable regression postulating an "additive model" [Hodge and Treiman 1966]), and on other countries than the United States (e.g., Sweden [Seeman, Rohan, and Argeriou 1966]). Nevertheless, the proposition that mobility increases prejudice remains a more or less accepted part of the sociological lore on the "structural" factors in ethnic hostility (Bettelheim and Janowitz 1964; Leggett 1968, p. 93). The proposition may yet

[1] I am deeply indebted to Richard T Morris, Phillip Bonacich, James Bishop, Jerald Schutte, and Samuel J Surace for critical commentary on an earlier draft and for assistance in the data analysis The work was supported by funds from the National Institutes of Health (MH 10460) for comparative work on alienation, and from the U.S. Department of Labor Manpower Grant to UCLA (#31-05-70-17).

be shown to be true (certainly the evidence is not wholly negative); but convincing positive evidence is not in hand.

The same doubts hold concerning the impact of mobility on variables other than racial prejudice. Recent studies suggest that mobility may have some effect on the extent of family contact that an individual maintains (Kessin 1971) and on reduced fertility rates, though on the latter point Blau and Duncan (1967) add that "occupational mobility, in general, is not a very productive variable for purposes of demographic analysis" (p. 392). A presentation showing that downward mobility in Italy is associated with leftist politics (Lopreato and Chafetz 1970) has been followed by an analysis arguing the contrary: "mobility *per se* does not appear to affect the political affiliation of the socially mobile in Italy" (Jackman 1972, p. 713).

The disarray in this field of investigation is partly the result of differences in methodology, but two major substantive factors also complicate matters. They are what Germani (1966, p. 369) calls the "degree of discontinuity" among social strata, and the "mobility values, attitudes and beliefs" in the society or sector under consideration. The former term refers to differences in the rigidity or visibility of the cleavage among classes; the latter signals the importance of prevailing attitudes about mobility and individual achievement These two features are commonly taken to be conditioners of the mobility experience (i e , moderators of its hypothesized effects). Yet despite considerable relevant commentary on these matters, we have little direct evidence concerning either the structural effect of rigid versus more penetrable class lines or the conditioning imposed by established attitudes regarding the importance of individual mobility

The suppositions concerning the effect of structural discontinuity are illustrated in the argument by Bendix and Lipset (1959, esp. chap. 2) that the wider discrepancy in consumption styles between the European middle and working classes leads the upwardly mobile European to suffer greater difficulty in adjusting to his higher status than does his American counterpart. Therefore, purportedly, his sense of relative deprivation or discrimination is greater and his political radicalism is encouraged. As Simpson (1970) notes, the stress which supposedly produces these results for the upwardly mobile in the more ascriptive societies can derive either from the greater stigma (hence, lesser acceptability) that attaches to lower class origin or from the greater difficulty of learning new class ways where the subcultural differences are marked.

Similarly, suppositions concerning the relevance of mobility attitudes are prominent in discussions of downward mobility. Thus, one might expect to find more acute resistance to the status implications of downward mobility in societies "where success values are stressed and the belief in an open-class structure is widespread" (Lopreato and Chafetz 1970, p. 440).

The consequence of such differential resistance based on differences in mobility attitudes might be that racial prejudice among the downwardly mobile is exacerbated only (or especially) in status-oriented societies The sense of personal responsibility for status decline, with all that this responsibility might imply, would also be high in such societies. In societies which are less achievement-minded, the occupational "skidder" would presumably find alternative means of accommodating to his new (low) status position. Among the alternatives frequently mentioned are the adoption of a left-wing political stance or identification with a labor movement (and a working-class perspective) seeking group betterment rather than personal status defense.

Although such concerns have been discussed in the literature, it is not often that these supposedly powerful conditioning factors—in particular, the structural difference of class "discontinuity," and the attitude difference concerning social mobility—have been jointly and directly examined. To do so is the purpose of the present paper It is designed to examine: (1) the contrasting effects, if any, of occupational mobility in France and in the United States, the former being taken as a society whose class structure is significantly more "discontinuous" (Hoffman et al 1963) compared with "the diffuse class boundaries in the United States" (Blau and Duncan 1967, p. 398); and (2) the bearing of mobility attitudes on any observed differences between the United States and France in mobility effects "Mobility attitudes" are defined as the relative value that individuals place on success and achievement

As the discussion thus far suggests, the number of variables on which differences might be expected (between mobiles and stables, and or the mobility-minded vs the less success-minded, both within and between countries) is quite extensive, and evidence on a wide range of such variables was obtained in the present investigation. These variables, and the expectations concerning them, are detailed below, but the main thrust of the analysis can be sketched as follows

1. If upward mobility is the problem it is often said to be, producing status insecurity and isolation, the greater difficulty of social acceptance in the more discontinuous French society should make upward mobility more psychologically difficult there This proposition translates into an expectation that the upwardly mobile in France will exhibit a greater sense of relative deprivation, greater political resentment (of the extreme left or right), and a variety of intensified alienations (e g , a greater sense of social isolation, more powerlessness) relative to their American counterparts.[2]

[2] As with most predictions concerning the effects of upward mobility (e g , that it will be associated with high ethnic prejudice or with conservative politics [Greenblum and Pearlin 1963, Tumin 1967]), a contrary argument can easily be developed—a fact which may help to account for the indeterminacy which characterizes the literature

2. Downward mobility, too, ". . . may be expected to have more severe effects where discontinuity and hierarchization are high" (Germani 1966, p. 369). At least, such mobility should produce different, if not more severe, reactions in France as compared with the United States. On the whole, one might expect a greater tendency to personalize status decline in the United States--for example, a tendency (*a*) to "internalize" occupational skidding (blaming oneself rather than the system for failure), (*b*) to maintain social distance vis-à-vis the "lower" class (avoiding the occupational "community"), and (*c*) to express the status hurt in sharpened hostility toward minority groups. The French case, on the other hand, should produce more politicized and class-conscious orientations of the left or right.[3]

3. Whatever the fate of these predictions concerning a person's objective mobility history (and there are grounds for doubt, as we have seen, that any such mobility effects will be found), it was expected that attitudes toward mobility would significantly condition the consequences of mobility in the two societies. The United States is typically held to be more oriented to individual striving than France and other European countries, and the first problem was to document this postulated difference through the direct measurement of mobility attitudes. More important, however, it was expected that status-mindedness would function similarly in the two societies: *both downward and upward mobility should be most problematic among those scoring high in mobility orientation*. Thus, it was anticipated that, in both countries, the supposed high prejudice of the downwardly mobile would be found only among those who value mobility highly; among the upwardly mobile, too, the supposed frustrations and anxieties of competitive upward mobility should be most intense among the more mobility-minded mobiles.

SAMPLES AND METHODS

The data derive from interviews conducted in Los Angeles and in Paris in 1966 and 1967, respectively. In each country, roughly 450 employed

seeking to demonstrate mobility effects. In the present case, the contrary argument would focus on the potentially *positive* consequence of being occupationally successful in a relatively "tight" society. The cast of this paper's introduction should alert the reader to the fact that I am here following, without endorsing, the traditional argument which emphasizes the status insecurity generated by mobility. It is precisely because of these potential contradictions regarding the effects of movement that it is helpful to have what is provided here namely, the control over individual attitudes regarding mobility, and the comparative evidence from both the United States and France.

[3] Mattei Dogan (1967, p. 173) has commented concerning France: "The downwardly mobile . . . very often remain attached to the social status and the symbols of their former class but, strongly frustrated, express Extreme-Right tendencies." Concerning Italy, Lopreato and Chafetz (1970, p. 441) remark: ". . . skidders surpass the old-timers of the working class in the frequency of both left-wing and conservative preferences. The leftist edge, however, is more pronounced than the conservative edge."

male workers, in occupations throughout the spectrum of manual and non-manual work, were interviewed. In the United States, respondents were obtained by random sampling within neighborhood blocks which had been selected so as to represent the range of socioeconomic status while establishing controls for ethnic membership (avoiding Oriental, Negro, Mexican-American, and Jewish concentrations). In France, the respondents were obtained by quota sampling (both the sampling and the interviewing having been done by a well-known French polling agency)[4] so as to reproduce the population characteristics of the work force in the Paris area (avoiding minority members, retired workers, students, and others not regularly employed).

The interview guide was substantially the same in the two countries, which is to say that it covered the same variables but made the necessary cross-cultural adjustments. Three examples of such adjustment follow.

1. In both countries, an objective true-false test of political knowledge was administered, concerning domestic and international affairs. The tests in the two countries obviously had to be quite different in their specifics, and though both of them were pretested it is not possible to assert that their level of difficulty was the same in the two countries. The issue of direct comparability of scores, however, is a minor one, since we are more interested in the *relative* scores (and/or correlations) attained by French subgroups which vary in their mobility history as compared with those attained by American mobile and stable subgroups.[5]

2. Ethnic hostility in the United States was measured by a series of Likert-type items bearing on anti-Semitic and anti-Negro attitudes, but in France a social distance format was used inquiring about the acceptability of four minorities (Algerians, Jews, Negroes, and Americans).

3. The political "left" has a unique importance in France as compared to the United States, and, as we have seen, the left-right distinction is a major focus of interest in European mobility studies. In the French interview, respondents were asked to classify their own political tendency on a continuum ranging from the "extreme left" to the "extreme right",[6] but in

[4] I am indebted to Pierre Weill of SOFRES (Société Française d'Enquêtes par Sondage) for his direction of the fieldwork and for assistance in pretesting, interviewer training, and translation.

[5] The point is parallel to the suggestion (by Duncan 1966, p. 95) that "... in principle, it is easier to advance our knowledge of the 'consequences of mobility' than to establish similarities or differences in respect to 'patterns of mobility' in a comparative framework," since direct comparability of indices is not so essential in the former case.

[6] The seven-point left-right scale was taken from Deutsch, Lindon, and Weill (1966), whose repeated studies between 1964 and 1966 have shown that a steady 30% of the French electorate classify themselves as adherents of the "center." A comparable figure was obtained in the present sample, suggesting that, in that respect, our Parisian workers constitute a reasonably representative group: 30% of our manual workers and 31% of the white collars called themselves centrists.

the United States, the closest parallel was the respondent's report on his vote for or against the open-housing proposition (proposition 14) on the California ballot (a "no" vote, against repeal, being the "liberal" position).

Beyond these "adjusted" measures, there were a number of more or less directly translatable measures, chief among these being a variety of alienation indices, and an index of mobility orientation. The alienation measures included the following:

1. A 13-item forced-choice scale of "powerlessness," containing items that have been effective in earlier studies and are derived from the index of Internal-vs.-External Control (I-E scale) developed by Rotter (1966). A sample of the choice offered to the respondent: (*a*) Becoming a success is a matter of hard work; luck has little or nothing to do with it; or (*b*) Getting a good job depends on being in the right place at the right time

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3. The Middleton (1963) alienation scale, composed of six items (one for each of six varieties of alienation [see Seeman 1972*a*]). The items are presented in a "strongly agree" to "strongly disagree" format Two of Middleton's items were of most interest (since we had better multiple-item measures of powerlessness and other alienations)· for cultural estrangement, "I am not much interested in the T.V. programs, movies, or magazines that most people seem to like", and for social isolation, "I often feel lonely."

4. A seven-item index of work alienation—a measure of self-estrangement in work developed through factor analysis and described in greater detail elsewhere (Seeman 1967) Sample items (all in yes-no format): "Does your work really give you a chance to try out your own ideas or not?"; "Is your job too simple to bring out your best abilities, or not?"

5. A three-item measure of the respondent's participation in an "occupational community" (indicating whether he had close friends at work, or in the same occupation, with whom he had more than occasional off-the-job interaction).

Another major line of inquiry, beyond the several alienation and political variables just described, concerned the respondent's "mobility orientation" Though rarely directly measured in cross-national work, differences in mobility orientation (e.g., the success orientation of Americans vs. others, or of variously mobile groups) are often assumed to be consequential in determining the response to actual mobility. This is seen in the proposal by Lopreato and Chafetz (1970, p. 450) that "if skidders hold

a strong success ideology, they are likely to resemble their class of origin in political behavior," or Wilensky's (1966, p 125) proposal that in poor countries where a strong success ideology is lacking ". the response to intergenerational or work-life 'skidding' is externalized; it is readily channeled into collective political protest."

Our measure of mobility orientation was composed of 14 items (in agree-disagree format) aimed at determining whether occupational advancement was a high priority value for the respondent. These items basically posed a choice between occupational success and other (presumably more "intrinsic") values Two examples. (1) "I'd probably turn down a substantial advancement if it involved being away from the family a good deal" (agree = low score); (2) "I would probably turn down a position that would allow me less freedom to express my views on political matters" (agree = low score).[7]

One final methodological point: this study is most directly concerned with the fact of mobility and with attitudes toward it, and for both of these it utilizes direct measures for the two countries I cannot undertake here, however, to demonstrate the postulated difference in class discontinuity between France and the United States. nevertheless, it is a characterization that is common in the stratification literature, both early and late (Bendix and Lipset 1959, Bertaux 1971; Blau and Duncan 1967; Giddens 1973; Hoffman et al. 1973; Hoffman 1974) I, too, assume that subtle and not-so-subtle differences in class relations remain, expressed in everything from dress styles and language to educational exclusiveness and political cleavage. Hoffman (1974), who is perhaps the dean of American analysts of French society, has captured the notion of discontinuity in his commentary on the roots of the "events of May" in 1968, those roots including a still viable inheritance of "old aristocratic values" embedded in "a hierarchical society with a large peasant reservoir, careful social distances between the various strata . and a clearcut separation between the workers and other elements of the population" (p 133)

[7] The best evidence on the psychometric properties of the scales described here is provided through their earlier use in a variety of contexts over a considerable period of time (on the powerlessness measure, e g, see Robinson and Shaver [1973], Rotter [1966], and Seeman [1967, 1972*a*]) Most of the scales used have an extensive history of that kind— including the "occupational community" measure which derives from the well-known study of union democracy by Lipset, Trow, and Coleman (1956), the "mobility attitude" scale (see, e g, Silberstein and Seeman 1959, Neal and Seeman 1964, and Seeman 1958), and, of course, the Srole "anomia" index which dates back to 1956 For each of the major scales (ignoring single-item indicators like class identification, voting behavior, or left-right political stance), we found that the reliabilities (using the Kuder-Richardson formula 20 [see Guilford 1954, pp 381–83]) were quite similar in the United States and France Thus, for political knowledge scores the reliability estimates were 68 and 69, respectively, for ethnic hostility ("prejudice," measured somewhat differently in the two countries), 59 and 77, for work alienation, 51 and 50, for powerlessness, 61 and 59, and 54 and 51 for anomia

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Still, though it is clear enough what the idea of "discontinuity" is pointing to—for example, to the fact that in France "access to higher education is still minimal" (Hoffman 1974; Bourdieu and Passeron 1964); or to the fact that the separation between "them" and "us" is more clear-cut in France during negotiations between labor unions and management (Crozier 1964) or concerning the professions where "admission to the club is closed to newcomers" (Crozier 1973, p. 172)—there is no guarantee that this generally postulated discontinuity is more than a carefully nurtured (or, at least, partial) myth[8] Testing that matter is not our task here; we wish simply to know whether in the two societies that are widely presumed to be different in this class-based way we find different and predictable results of class-related mobility.

MOBILITY ATTITUDES IN FRANCE AND THE UNITED STATES

Since the subjective side has been less well attended to in the conventional mobility studies, especially in the implicitly comparative ones, the first task was to examine mobility attitudes in the two countries, seeking to discern not only their degree of similarity but also their relation to the supposed consequences of mobility. There was every reason to expect some radical French-American differences in mobility attitudes—either in their structure or in the level of commitment to striving values. At least, that is the accepted wisdom concerning the urgency of success and striving in the United States. But just as many have grown cautious about the myth that objective mobility rates are notably higher in the United States than in Western Europe, it may be that more caution is in order on the subjective side as well, for the data show a striking similarity between French and American mobility values.

The analysis began with the thought (though not exactly the hope, given some earlier experience with Swedish data [Seeman et al. 1966]) that Guttman scaling would be useful in revealing the differential structure of mobility attitudes in the two countries. That thought was encouraged by the very conception of the mobility attitude instrument: it is a measure of what Rotter (1972) would call the "reward value" of occupational striving—its place in a hierarchy of values, with the competing values (in the scale items) being family, community, health, friends, peace of mind, political freedom, independence or creativity at work, and the

[8] We have had the experience of discovering such myths in a related area—e.g., the myth that American rates of occupational mobility are distinctly different from European ones. Even that question, however, is not exactly closed. In his recent reevaluation of the evidence concerning mobility in France, Bertaux (1971) has capsuled his conclusion as follows: "It is clear that the principal phenomenon which characterizes 'social mobility' in France is the immobility" (p. 91); see also Garnier and Hazelrigg (1974).

like. It seemed eminently reasonable to expect that these values are crystallized into some more or less standardized hierarchy of preference or worth (e.g., health is a high-risk value; one's family is more important than friends); and that the hierarchy of values might well vary in the different cultures of France and the United States.

The analysis, however, did not corroborate these expectations. The mobility attitude items failed to produce a scalable Guttman pattern in either France or the United States. This negative outcome held for manual and white-collar workers (in each country) treated separately and together, and for scales of varying item length (ranging from eight items to a bare four items).[9]

This negative similarity between France and the United States, instructive but not very illuminating in itself (since the items could fail to scale for various reasons and the failure thus hardly establishes that differences do not exist), is consistent with more positive evidence of similarity. The percentage of endorsement (those choosing the mobility-oriented alternative) is shown in table 1, for French and American respondents, controlling both for occupation and for education The most striking feature in table 1 is the similarity between the French and American workers in their degree of mobility-mindedness, a similarity which holds for the well educated and the poorly educated, for manuals and white collars. At least, that is so for the first six values presented in table 1. The last two values, however, ("peace of mind" and "family") produce radical differences between the two countries--but again, very consistently so On these items, in the four repetitions that table 1 provides, the French workers (regardless of occupation and education) are much *more* ready to take the promotion even though it means being away from the family a good deal or may be costly in terms of peace of mind. These two basic findings—that is, the fundamental cross-national similarity of endorsement, and the French lack of interest (relatively) in family "togetherness" and job tranquility—certainly do not speak very strongly for the supposed urgency of striving values in the United States; if anything, these data show the French to be more committed to striving.

Still, it may not be so much a matter of "more" commitment as of "different" commitment, of differently structured mobility attitudes in the two societies. To examine that possibility, the mobility attitude items were

[9] The eight-item scale involved the values enumerated in table 1 The best that could be managed in 18 tries at scaling was a coefficient of reproducibility of 86 with a minimum marginal of 75 (percentage improvement, 0 11), obtained with four items among the American white-collar workers The more general items, those not posing a value choice and representing what one might call "mobility ideology" (see items 9–12 in table 2), were not included in the Guttman analysis (e g , the item which read, "I've more or less had a long-range plan for myself, and moving every now and then to get new experience and a better job is part of it").

TABLE 1

PERCENTAGE ENDORSING THE MOBILITY-ORIENTED ALTERNATIVES, FOR FRENCH AND AMERICAN SAMPLES, WITH OCCUPATION AND EDUCATION CONTROLLED

VALUES COUNTERPOSED TO MOBILITY	MANUAL U.S	MANUAL France	WHITE COLLAR U.S	WHITE COLLAR France
	($N = 61$)	($N = 136$)	($N = 107$)	($N = 151$)
Low education:				
1. Being a community "stranger"	72	73	85	89
2. Political free expression	71	64	43	47
3. Increased job responsibility	67	63	96	82
4. Restriction on job creativity	43	41	18	22
5. Postponing having children..........	34	44	29	34
6. Endangering health	20	13	26	26
7. Peace of mind	8	43	36	75
8. Being away from the family	18	38	22	46
	($N = 95$)	($N = 76$)	($N = 132$)	($N = 125$)
High education:				
1. Being a community "stranger"	69	84	80	78
2. Political free expression	59	53	56	60
3. Increased job responsibility	86	71	91	65
4. Restriction on job creativity	36	29	39	38
5. Postponing having children	38	37	34	42
6. Endangering health	20	20	20	13
7. Peace of mind	17	58	24	55
8. Being away from the family	31	53	22	43

NOTE: Items 7 and 8 yield consistent and substantial differences (over 20%) between French and American samples regardless of occupation or educational level. Four items which did not pose specific value contrasts (the value of mobility vs. a particular value—e.g., family, health, etc.) and hence may be designated as "mobility ideology" items are omitted from this table (see table 2, items 9-12).

factor analyzed, with the results indicated in table 2, where the data for the white-collar workers are presented.[10] There are two notable features in table 2: the mean scores are reasonably comparable in France and the United States, and the factor patterns are remarkably similar for two countries that are commonly held to be so different in their attitudes about success.

The three factors that emerged are named in table 2, the similarity and clarity of the first two factors making their identification fairly straightforward: (1) The largest set of items define those who opt for the un-

[10] Factor analyses were conducted for the total sample in each country, and independently in each for the manual and nonmanual workers, with basically similar results despite minor item differences across the occupational line. Table 2 avoids unnecessary detail by focusing on white-collar workers (the larger sample) in both countries. The analysis involved a principal component solution with varimax (orthogonal) rotation. It bears emphasizing that (*a*) the factor analysis is not treatable as strictly independent of the Guttman results, these two procedures being based upon related though different assumptions and properties, and (*b*) the failure to produce satisfactory Guttman scales is reported more as a matter of the record of effort in that direction than as a demonstration of similarity.

TABLE 2

MEAN SCORES (WITH THEIR STANDARD DEVIATIONS) AND VARIMAX ROTATED FACTOR MATRICES FOR MOBILITY ATTITUDE RESPONSES OF WHITE-COLLAR WORKERS IN FRANCE AND THE UNITED STATES

	France (N = 276)				United States (N = 239)			
Items	Mean Score (SD)	Factor I*	Factor II†	Factor III‡	Mean Score (SD)	Factor I*	Factor II†	Factor III‡
1. Would turn down an advancement if it meant *being away from the family* a good deal	2.95 (1.7)	*59*	00	— 39	2.28 (1.2)	*.52*	03	— 27
2. The thought of the *increased responsibility* would inhibit moving up	3.96 (1.4)	*62*	09	17	4.15 (1.0)	47	—.18	.48
3 Favor staying with a job without prestige but a good bet for *peace of mind*	3.50 (1.6)	*.74*	— 14	22	2.33 (1.2)	*67*	.08	.24
4. Wouldn't take promotion, no matter how big an improvement, if it meant endangering *health*	1.90 (1.4)	*63*	06	— 17	2.13 (1.1)	*.55*	.23	— 25
5 Wouldn't let being a "*stranger*" keep me from moving to a higher position elsewhere	4.26 (1.2)	46	27	56	3.76 (1.0)	.21	—.41	18
6 Would turn down a position allowing less *freedom to express political ideas*	3.59 (1.5)	03	*67*	13	3.11 (1.3)	—.07	*73*	25
7 Would turn down a higher job where you couldn't *try out your own ideas*	2.63 (1.6)	13	*.74*	— 09	2.54 (1.2)	— 04	*.81*	—.12
8. Willing to delay *having children* to take advantage of the opportunities for advancement	2.90 (1.6)	— 11	— 05	— 29	2.59 (1.2)	03	.33	—.06
9 The man who says he isn't out to "get ahead" is kidding himself or trying to kid others	3.96 (1.4)	— 21	— 20	*41*	3.84 (1.0)	01	—.08	*70*
10 Those who stay in small jobs many years would probably not amount to much even if promoted	2.22 (1.3)	01	— 07	*32*	3.12 (1.2)	— 24	— 09	*.68*
11 Have a long range plan, moving now and then to get new experience is part of it	1.97 (1.4)	20	— 50	.09	2.69 (1.2)	47	—.21	.15
12 It's worthwhile to assure having a good name with the right kind of people	3.47 (1.5)	— 05	01	*52*	3.82 (1.0)	— 13	— 29	.01

NOTE Items 9-12 involve attitudes about mobility in general ("mobility ideology") Italicized items pose specific value contrasts (the value of mobility vs. the italicized value) The wording of the items is essentially as in the original but with slight abbreviation
* Careerist vs nonstriver
† Conformist vs independent
‡ Ideologue vs. unconvinced.

stressful life ("nonstrivers") as against mobility at any cost ("careerists"). The low scorers on this factor are those who choose family, peace of mind, modest responsibilities, community roots (not being a "stranger"), and personal health over climbing. (2) There emerges also a very modest "independence" factor, composed mainly of two items which identify those who are not willing to sacrifice their own political or work-related ideas for the sake of mobility ("independents") in contrast to the "conformists" who are willing to do so.[11]

Are these factors simply artifactual similarities between France and the United States, or do they signify similar processes at work in the two countries? One way to pursue this question was to develop factor scores and determine the relation of these factors to the main classes of variables that were available in both countries: (1) the background characteristics of the respondents (education, income and age); (2) the alienation scores (powerlessness, work alienation, and anomia); and (3) the political measures (class identification, ethnic prejudice, and political knowledge). If the argument developed thus far has merit, the attitude score on careerism versus nonstriving should correlate meaningfully with the dependent variables enumerated, and the pattern of these correlations should be reasonably similar in the two countries.

Table 3 shows that these expectations are realized in substantial degree. Thus, we find that (1) the careerists are the better-educated young people in both countries; (2) powerlessness (I-E scores) and work alienation do not correlate with careerism in either country— but the alienation measure which is more like an index of generalized discontent (the Srole scale) correlates with careerism in both countries (most notably, however, in France): the careerists, who are prepared to sacrifice various intrinsic values for mobility, are less alienated, while the "nonstrivers" (the workers who have more or less given up striving) are more alienated, this pattern holding across the manual white collar line, and (3) the careerist attitude is not closely tied to the political variables Again, the relationships are similar in the two countries. The careerist orientation tends to accompany low prejudice, and even with respect to social class identification, where the two countries might be expected to differ considerably, there is a basic

[11] The third factor (not nearly so clear in its own right in France, despite the general similarity) centers around two mobility ideology items High scorers view striving as a *typical* and laudable motive (the "ideologues") vs those with more reservations (the "unconvinced"), who deny (e g) that the absence of movement is equivalent to the absence of talent (item 10) The mobility ideology factor was not scored since this dimension showed the least clarity and reliability The factor scores for careerism (five items), produced by weighting the contribution of each item in terms of its factor score coefficient, are *obviously tapping* the clearest core of the mobility attitude items hence we focus on this factor score in what follows.

TABLE 3

CORRELATIONS BETWEEN SELECTED VARIABLES (BACKGROUND CHARACTERISTICS, ALIENATION MEASURES, AND POLITICAL OUTCOMES) AND THE FACTOR SCORES ON MOBILITY ATTITUDES (CAREERISM VS NONSTRIVING)

	MANUAL		WHITE COLLAR	
	France ($N = 212$)	U S ($N = 157$)	France ($N = 277$)	U.S ($N = 243$)
Background variables				
Education	26*	22*	32*	14
Income	29*	10	22*	01
Age	− 39*	− 29*	− 32*	− 14
Alienation scores				
Powerlessness (I-E)	− 06	− 15	− 03	03
Srole anomia	− 35*	− 11	− 24*	− 19*
Work alienation	− 12	01	− 08	01
Political outcomes				
Class identification	− 18*	− 18	− 21*	− 14
Prejudice	− 18*	− 12	− 12	− 13
Political knowledge	17	03	19*	05

NOTE High scores on careerism indicate greater commitment to mobility and low scores indicate nonstriving attitudes High scores on class identification mean identification with the working class rather than the middle or upper class (other stub variables are standard in their scoring high scores mean high education, high alienation, high prejudice, etc).
* Significant at the 01 level or beyond

similarity: the careerists identify upward (low scores signify middle-class identification).[12]

In sum, we find that a variety of evidence speaks to the essential similarity of mobility attitudes in France and the United States—their similarity in structure (evidenced positively in the parallel factor structures and negatively in the mutual resistance to Guttman scaling), their similarity in degree (evidenced in the generally comparable levels of endorsement in the two countries), and their similarity in significance (evidenced in the parallel correlations of the factor scores with personal background, alienation, and political views).

THE CONSEQUENCES OF MOBILITY

Does this similarity hold as well for the supposed effects of mobility itself? The customary argument is that these effects should be different in the two countries—for example, that the sense of relative deprivation and social

[12] As noted, the careerists are younger and better educated, but the partial r's controlling for age and education do not change the r's in table 3 in important degree—e.g , the partial r between careerism and anomia, with age controlled, is − 28 (as against − 35 in table 3). The difference between the parallel French and U S correlations has been tested, and of the 18 pairs in table 3, only one (− 35 vs − 11 for the Srole scores) is significantly different at the .05 level

isolation should be more characteristic of the upwardly mobile in France, where the resistance to full acceptance in the new class is presumably greater. For the downwardly mobile, also, cross-national differentials are predicted: in the United States, a predominance of "personalized" ways of handling the status decline (e.g., retreat from politics and ethnic hostility) compared with more politicized reactions in France.

Though such effects have been predicted, it must be said that the predictions in the literature are not notable for their overall consistency (see Segal and Knoke [1968] and Thompson [1971] for recent illustrations on that score). The inconsistency stems in part from assumptions (often implicit) about the striving attitudes of the subgroups involved. For that reason, and because we have already seen that the major mobility attitude factor extracted in both the United States and France (careerism vs. nonstriving) correlates significantly with a number of background factors and other variables (see table 3), mobility attitudes as well as mobility history are accounted for by the data of tables 4 and 5, where some likely outcomes of mobility are examined. Table 4 deals with the careerists (those

TABLE 4

Mobility Effects among Careerists: Adjusted Mean Scores and Analysis of Covariance Results (Education Controlled) for Different Mobility Categories among French and American Workers

	France				United States			
	Manual		White Collar		Manual		White Collar	
Outcome Variable	Mobile Down	Non-mobile	Non-mobile	Mobile Up	Mobile Down	Non-mobile	Non-mobile	Mobile Up
1. Work alienation	10.1	9.2	9.2	9.7*	9.6	8.8	8.6	8.6
2. Relative deprivation (Srole item)	3.2	3.4	2.5	3.2	2.5	2.0	2.2	2.2
3. Srole anomia	13.8	13.5	12.4	13.9	10.9	10.6	10.1	10.7
4. Powerlessness†	17.5	20.3	20.9	18.3	19.0	19.2	21.2	20.4
5. Political knowledge†	8.4	7.9	8.7	8.4	18.8	20.0	20.5	20.4
6. Prejudice†	25.9	25.5	26.5	26.0	21.7	18.7	19.2	24.7
7. Occupational community	6.4	5.9	6.9	6.8‡	4.1	4.3	3.8	4.5
8. Social isolation (Middleton item) ..	1.6	1.8	1.9	1.9	1.9	1.8	2.1	1.8
9. Cultural estrangement (Middleton item) ..	3.6	3.0	3.3	2.8	2.7	2.5	3.1	3.0
10. Job satisfaction§ ...	2.7	3.1	3.0	2.9*	2.8	2.9	3.0	3.2
N cases	35	60	99	35	26	22	63	35

* Indicates a significant interaction (.05 level or beyond) between respondent's occupational origin and his present occupational level—i.e., a mobility effect.

† These scores were not directly comparable across countries, since the tests involved were not administered in precisely the same way in France and the United States (e.g., different items concerning political knowledge, different formats for prejudice, etc.)

‡ Indicates a significant main effect for respondent's present occupational status (an *F*-ratio significant at the .05 level or beyond)—i.e., a difference between manuals and white collars.

§ *Job satisfaction* was measured via a single item, scored on a four-point scale (high score = high satisfaction).

TABLE 5

MOBILITY EFFECTS AMONG NONSTRIVERS ADJUSTED MEAN SCORES AND ANALYSIS OF COVARIANCE RESULTS (EDUCATION CONTROLLED) FOR DIFFERENT MOBILITY CATEGORIES AMONG FRENCH AND AMERICAN WORKERS

	FRANCE				UNITED STATES			
	Manual		White Collar		Manual		White Collar	
OUTCOME VARIABLE	Mobile Down	Non-mobile	Non-mobile	Mobile Up	Mobile Down	Non-mobile	Non-mobile	Mobile Up
1 Work alienation	11.0	10.0	9.5	9.3‡	9.1	9.6	8.8	8.9‡
2. Relative deprivation	3.6	4.0	2.7	2.9‡	2.7	2.4	2.3	2.1‡
3 Srole anomia	16.8	16.6	14.9	12.8‡	15.3	12.4	11.3	11.3‡
4 Powerlessness† . ..	22.8	17.8	20.5	20.4	20.3	19.5	22.2	19.7
5 Political knowledge†	6.2	7.6	8.5	7.8*‡	19.0	18.4	20.4	19.7‡
6. Prejudice†	30.6	27.6	26.4	25.9	23.5	19.9	20.5	24.3
7 Occupational community	6.3	6.3	7.4	7.0‡	4.4	4.5	3.5	4.1‡
8 Social isolation	2.2	2.1	2.0	1.8	2.0	1.9	1.9	2.0
9 Cultural estrangement	2.8	2.7	3.0	2.8	2.9	2.9	3.0	3.3
10 Job satisfaction§ ...	2.9	2.9	3.2	3.0	3.0	2.9	3.0	2.6
N cases	30	49	72	31	44	68	88	50

NOTE — See table 4 for explanatory notes.

scoring high on the five-item factor previously described), and table 5 presents the same analysis for the nonstrivers (the low scorers on that factor, who indicated a relative unwillingness to give up a variety of values in the interest of occupational mobility).

Both tables 4 and 5 have been constructed on the basis of a series of two-way analyses of covariance (manual vs white-collar occupation of father × manual vs. white-collar occupation of son, controlling for the respondent's education). Thus, the data in table 4 on (for example) work alienation in France effectively present the mean scores (adjusted for the educational covariate) and the results of their covariance analysis as follows:

	FATHER'S OCCUPATION	
RESPONDENT'S OCCUPATION	White Collar	Manual
Manual	10.1 (mobile down)	9.2 (nonmobile)
White collar	9.2 (nonmobile)	9.7 (mobile up)

The main interest in these analyses of covariance is in the demonstration that there is (or is not) a significant interaction term—that is, in whether there is a true mobility effect in the sense that one must take into account both the person's origin (father's occupation) and his present status (re-

spondent's occupation) to understand the outcome. Thus, as many have recently argued, the patterning of these mean scores is crucial. The argument for a mobility effect cannot rest upon a result which shows that the downwardly mobile or the upwardly mobile merely combine the two sources of their history--that is, have mean scores which simply lie between their class of origin and their class of destination (a result which only attests to dual socialization rather than to any specific dynamic of mobility itself). Nor, of course, does a difference, however large, between the manual and the white-collar workers (i.e., a main effect in the analysis of variance attributable to respondent's present occupation) tell us about the conse quences of mobility. Tables 4 and 5 present the detail for an examination of these three main points of interest for both the French and the U.S. workers: (1) most important, is there a significant interaction effect in the covariance analysis in either or both countries for a given outcome variable; (2) is there a significant main effect for current occupation (manual vs. white-collar workers in either country); and (3) is the pattern in the four mean scores for each country of the type that would be anticipated by simple socialization effects (what some have called the "additive model" [Duncan 1966]) rather than mobility effects?

The data in tables 4 and 5 are unavoidably complex, given the variety of outcomes examined, the treatment of the mobility attitude dimension as well as mobility history, and the provision for between-country comparisons. In view of the sheer number and range of potential differences, it seems remarkable how similar the cross-national portraits are. The main thrust of these data can be summarized as follows.

1. In both countries, the evidence for a specific mobility effect (i.e., patterned significant interaction) is very weak, indeed. In table 4 (i.e., for the careerists) none of the interactions for the American workers is significant; in France, what effect there is (two significant F-ratios for interaction) occurs only on work-related measures: there is greater work alienation and more job dissatisfaction among the downwardly mobile careerists. It is worth noting that the trend goes the same way in the United States—that is, the mean score on work alienation (9.6, see variable 1 of table 4) is relatively high for the downwardly mobile strivers. Among the nonstrivers (table 5), there is again no evidence of significant mobility effects in the U S. data; and one instance of significant interaction in France: the downwardly mobile French workers are notably low in their level of political knowledge. The striking fact, however, is that these data document what surely can be called minimal evidence for mobility effects: none of the interactions was significant for the U.S. case; and only three were in the French case (i.e., three significant interactions in a total of 40 analyses of covariance covering a range of outcome possibilities).

2. The idea that upward mobility in France is more problematic than

in the United States receives very little support. In both the U.S. and the French data, for both careerists and nonstrivers, the white-collar workers who have been "mobile up" have outcome scores that are generally very similar to their nonmobile white-collar counterparts and/or scores which stand (as one might expect on the dual socialization hypothesis) between those of the manual nonmobiles and the white-collar nonmobiles (effectively, their class of origin and their class of destination; see, e.g., the relative deprivation or political knowledge scores in tables 4 and 5).

3. The proposition that upward and downward mobility are alike in that they both disrupt established social ties and weaken the network of social bonds receives very little support as well. The alienation measures that speak to that question (chiefly, the occupational community index, the Middleton social isolation item, and, to some extent, the cultural estrangement item) show no such effect in either country.

4. While mobility per se appears to be relatively unimportant, present occupational status (the manual vs. white-collar distinction) shows more notable effects, and those are basically consistent in the two countries This is especially visible in table 5, where the significant manual versus white-collar differences in France are matched with similar differences in the United States. In both countries, as one would expect, the white-collar workers are less alienated at work, lower in anomia and in their sense of relative deprivation, and more knowledgeable in politics The measure of "occupational community" also shows a substantial manual-nonmanual difference in both countries, but the pattern is different in the two cases: the French manuals are low in occupational community, while in the United States the manuals score significantly higher than the white-collar group. But it should be noted that mobility per se is not involved: the upwardly mobile in both countries have scores that lie between their origin and their destination, and the downwardly mobile have scores which are not distinguishable from their stationary manual counterparts Thus, the conclusion would seem to be that, on the whole, the outcome measures appear to be "working" all right (producing reasonable and expectable differences), but true mobility effects are very limited, indeed, in both countries [13]

[13] The cross-country similarity commented on earlier with respect to mobility attitudes emerges in tables 4 and 5 as well; at least, these data suggest that "low" careerism (nonstriving) is similar in the two countries in being effectively equivalent to "disengagement". the manuals are more clearly distinguished from nonmanuals (and generally more alienated in the various senses of that term) among the nonstrivers (i.e., in table 5) It bears signaling, too, that in both countries the downwardly mobile nonstrivers (i.e., those who have "fallen" occupationally and are "disengaged" attitudinally) show some signs of special trouble in France and the United States, they score relatively high in both anomia and racial prejudice (though the interaction term in these instances is not statistically significant; see variables 3 and 6 in table 5) The careerism score has been emphasized here since it is the major factor, based upon a larger number of mobility attitude items. A similar analysis to that given in tables

In sum, tables 4 and 5 argue for a great deal of caution about the supposed effects of intergenerational mobility and the supposed difference in mobility effects as between the United States and France. On the whole, such mobility effects are either nonexistent (e.g., social isolation, powerlessness, and cultural estrangement in both countries), or explainable on an additive basis (e.g., relative deprivation in France), or limited in their extent (essentially limited, we have found, to work alienation and job satisfaction among the careerists in France, with the American sample following the same trend though not in statistically significant degree).[14]

Still, some other questions remain, notably one about the specifically political, or politically relevant, consequences that are often attributed to mobility in the two countries—for example, whether voting behavior and/or leftist inclinations are related to mobility, and whether this seems to be the case uniquely for France. Not surprisingly, it is difficult to derive strictly comparable measures in the political domain, and those that are here available were basically dichotomous in nature (e.g., voted or not), hence the data have been arranged (in table 6) in a simple percentage analysis, comparing the four mobility categories within the two countries.

It requires no formal or sophisticated mobility analysis (e.g., dummy variable or covariance analysis) to observe that the data in table 6 again do not speak very impressively for the conclusion that there are mobility effects in the political domain in either country. There are, to be sure, some striking differences in table 6, but as we have seen before (in the covariance analyses of tables 4 and 5) these are either (1) differences in the general *level* of endorsement in the two countries (e.g., the French are considerably more ready than the Americans to leave political matters to the experts), or (2) substantial manual-nonmanual contrasts in both countries (e.g., "working class" identification in France and the United States is understandably higher among manual workers).

Most especially, the findings do not support the prevalent views about leftist consequences in Europe for either the downwardly mobile or the

4 and 5 was conducted using the marginally identifiable three-item conformism-independence score. Though the details need not be presented here, the essential fact is that the analysis again reveals very limited mobility effects.

[14] For a similarly negative body of evidence regarding mobility effects, see the recent paper by Jackson and Curtis (1972), involving communities in the United States only. They use the method of dummy-variable analysis, suggested by Duncan (1966), which has more or less dominated the recent literature (see, e.g., Hodge and Treiman 1966). I have chosen to ask whether a somewhat different procedure with a similar intention (i.e., the analysis of statistical interaction with close attention to the pattern of mean scores involved) produces similar conclusions regarding the need to postulate a specific mobility effect. For a procedure similar to the one used here, stressing both the pattern of mean scores and a test of significance via the method of unweighted means, see Simpson (1970). The covariance analysis was conducted via the program outlined in Armor and Crouch (1972) which is based on the unweighted means procedure described by Winer (1962).

TABLE 6

PERCENTAGE HOLDING VARIOUS POLITICAL VIEWS AMONG WORKERS IN DIFFERENT MOBILITY CATEGORIES, BY COUNTRY

	France				United States			
	Manual		White Collar		Manual		White Collar	
Outcome Variable	Mobile Down	Non-mobile	Non-mobile	Mobile Up	Mobile Down	Non-mobile	Non-mobile	Mobile Up
1. Did not vote in previous election	16	14	20	17	28	25	10	16
2. Identifies with political "left"*	53	62	46	53	27	30	12	18
3 Identifies with "working class"†	67	79	10	36	33	27	4	1
4 Believes "experts" should make political decisions‡	70	73	73	62	40	43	48	48
5 Believes success depends on luck (not hard work)§ ..	40	47	40	46	25	20	24	22
N cases	65	107	171	66	65	87	148	85

* The self-identification item used in France (basically a rating scale ranging from extreme left to extreme right, see text) was not applicable in the United States where the percentages represent those who voted in the liberal direction on proposition 14 in California (i.e., pro-integration in housing).

† The American "class identification" alternatives allowed for a "lower middle" and "upper middle" choice, but the French interview did not.

‡ In both countries, the full text of this single item for "expert orientation," read, "Actually, the basic decisions on political and social questions in this country ought to be made by the specialists and experts."

§ This item on attributions concerning occupational success or failure was one of the forced-choice powerlessness items, involving a choice between (*a*) Becoming a success is a matter of hard work, luck has little or nothing to do with it, or (*b*) Getting a good job depends on being in the right place at the right time.

upwardly mobile workers (variable 2, table 6). There are differences on leftism in France among the several mobility categories (and in the United States as well, though the two indices are not at all comparable; see n. *, table 6); but the differences are strictly in accord with the socialization ("additive") principles. The manual sons of manual workers are the most left-leaning group; the white-collar sons of white-collar workers are least so. The two mobile groups reflect both their occupational origin and their destination with percentages that lie between the stable categories.

The other variables in table 6 go generally in the same way, showing either minimal differences and/or differences that require no "mobility effect" by way of explanation. Thus, there is little evidence here for the view that both upward and downward mobility "increase political cross-pressures which bring about a reduced interest in politics" (Lopreato and Chafetz 1970, p. 441).[15] Nor is there evidence for the view that the down-

[15] A direct way of testing for political interest was also available (beyond the non-voting data shown in variable 1 of table 6), since we asked each respondent to rate

wardly mobile workers in either France or the United States require an "external" (luck not personal responsibility) explanation for their status decline. Neither the item on work in table 6 (variable 5) nor the total scale on powerlessness (from which the "luck" item is derived) provides support for the view that French society is one in which ". . . the response to intergenerational or worklife 'skidding' is externalized" (Wilensky 1966, p. 125). Though French workers do choose the "external" alternative much more heavily (roughly 42%, compared to 23% in the American sample), that choice has very little to do, in either country, with rising or falling occupationally. A similar point can be made concerning the dependence on experts in political decision making (variable 4, table 6): the item, as expected, is heavily endorsed by French workers and gets less than a majority in the United States, but in both countries mobility is irrelevant to the workers' stance on the issue

One further point is in order concerning the leftist orientation in France. One might suspect that the sense of relative deprivation, produced either by downward mobility (via the status fall that is resented) or by upward mobility (via a status climb that a tight society makes difficult), would encourage a lean toward the left. The data already presented (esp. variable 2 in tables 4 and 5 on relative deprivation, and variable 2 in table 6 on leftism) encourage some suspicion that such a mobility-deprivation syndrome does not account for the leftist stance in any important degree. Table 7 confirms that suspicion. It shows that the sense of relative deprivation does correlate in the expected way with self-identification to the

TABLE 7

PERCENTAGE CHOOSING A LEFT (VS CENTER OR RIGHT) POLITICAL IDENTIFICATION IN FRANCE, BY LEVEL OF RELATIVE DEPRIVATION, FOR WORKERS IN DIFFERENT MOBILITY CATEGORIES

	Manual				White Collar			
	Mobile Down		Nonmobile		Nonmobile		Mobile Up	
	Low D	High D	Low D	High D	Low D	High D	Low D	High D
% left	46	58	51	69	43	51	52	54
N	26	36	39	61	109	51	31	26
Gamma	−0 24		−0 35		−0.16		−0 04	

NOTE D = deprivation, low scores indicate a leftward orientation (hence a negative correlation reflects an association between high deprivation and left identification)

his own degree of interest in various activities (e g , discussing his work, keeping up with international affairs, etc); here, too, the postulated cross-pressures leading to low interest fail to materialize E g., in the French sample, the percentages showing low interest in international politics were (respectively, for the mobility groups shown in table 6) 46, 49, 28, and 36.

left, but that mobility has very little to do with the tie between deprivation and leftism. Indeed, among the upwardly mobile, deprivation has nothing to do with the quite substantial leftist inclinations that are found among these white-collar workers. Among the downwardly mobile, deprivation and leftism are associated in about the same way as they are for the stable manual workers (in fact, deprivation is somewhat less typical and less closely tied to leftism among the downwardly mobile). The proper conclusion would seem to be that, though the sense of deprivation plays some part in being politically leftist in France, mobility plays no special part in developing or encouraging the tie that binds these sentiments.

CONCLUSION

American sociologists seem committed to status-centered theorizing—committed, for example, to the dubious notion that climbing aspirations pervade the American class structure and to the even more dubious notion that status-defined success is equivalent to positive self-definition (for some doubts on these matters, see Porter [1968] and Seeman [1958]). The stubbornly held view (remarked upon at the outset of this paper) that social mobility is a profoundly disturbing affair is part of the same theoretical mix which stresses status seeking and status concern.

The present data are certainly not supportive of these convictions about the significance of status change. Departing from established notions about, first, the centrality of mobility-mindedness in the American scene and, second, the relative discontinuity of the French social order compared to the American (i.e., the more ascriptive and hierarchical character of the former), the analysis sought to document differences in mobility attitudes and in the effect of mobility itself in the two countries. The results, we have seen, are more impressive for their similarity than for the differences they document; they are also more impressive for the limit rather than the scope they establish for the effects of mobility in both countries.

The main findings can be summarized as follows

1. With regard to mobility attitudes, the findings confirm Porter's judgment: "Comparative studies have shown that upward mobility is a fundamental process in all industrial societies. They would probably also show great similarities in the distribution of ambition, mobility values and levels of aspiration" (1968, p. 11). So far as it goes, the evidence here shows such similarity between France and the United States (*a*) in the overall level of commitment to striving, (*b*) in the structure of these mobility attitudes, and (*c*) in the correlates of a striving orientation—that is, the young, activist, unalienated quality of the careerists.

2. With regard to mobility history, we find that the effects of mobility are extremely limited. Using both an analysis of covariance technique with

educational level controlled, and a simpler percentage comparison, to examine a wide range of potential correlates of mobility, the evidence is that most of these variables (e.g , powerlessness, voting behavior, social isolation, etc.) do not produce differences between mobility groups. These negative results, occurring in both France and the United States, raise further questions about the generality of mobility effects reported elsewhere (e g , in Latin America [Simpson 1970]), and about the pervasiveness of mechanisms that are thought to be central in producing such mobility effects (e.g., the supposed disruptive social isolation that mobility entails; and the postulated, but here undocumented, personal adjustment devices involved—e.g., blaming society for one's status loss).

3. Where statistically significant differences do appear (e.g., leftist identification in France, and liberal voting behavior in the United States) they generally show the downwardly mobile and the upwardly mobile groups falling, as one might predict, between the two occupational levels from which these mobile workers came. These are differences that are explainable on an additive rather than an interactive principle (i.e., as simple socialization rather than mobility effects).

4. Though there are differences, to be sure, between France and the United States, there is an overall similarity which is impressive, given the going assumptions concerning their dissimilarity in class structure and status attitudes. In general, the same variables show manual versus nonmanual differences (e g., anomia and political knowledge), and the true mobility effects are limited in both countries. Such mobility effects are more or less restricted to the work sphere in France, high work alienation and low job satisfaction being found among the downwardly mobile strivers (the American mean scores go in the same direction, but not significantly so)[16] What difference there is between the two countries suggests that downward mobility is more problematic in France (chiefly among those whose mobility attitudes are careerist), and that upward mobility there (contrary to some beliefs) is not.

[16] The work-related unhappiness of these mobility-minded "skidders" in France may be related to the fact that they have their fathers' white-collar positions as a reference point, and their decline takes place in a society which tends to see occupational horizons and resources as relatively limited they have come from the white-collar world, and have entered a French manual workers' world that is characterized by discontent Thus, if one takes the Srole scale to be an index of discontent (McDill 1961), it seems clear that (true to form) the French workers express more such discontent than do the American workers (table 4, variable 3) The data presented here appear generally to speak more to such French discontent than to a downward mobility effect (for earlier evidence on French blue-collar discontent, see Durand [1966], esp p 283) It bears repeating that the present data were collected less than a year before the explosion of May 1968, and the level of French discontent is presumably not irrelevant to that incident (see Seeman 1972b).

There are a variety of "could be's" that suggest themselves in the face of these negative results. With sundry variations, these amount to expressions of doubt regarding the two matters that have been of most concern from the outset: we are left to wonder, first, whether the supposed difference in class discontinuity between France and the United States is more myth than reality (at the very least, not a true enough portrait to produce consistent or significant differences in mobility consequences) and, second, whether the force that is so regularly attributed to mobility and to attitudes about mobility is still another myth (at least, an exaggeration of the effect of status concerns).

With regard to discontinuity, the similarity we have found may merely reflect the fact that the French sample involved workers in the Paris region--that is surely a special part of France, and our results may attest to the familiar proposition (Inkeles 1960) that similarly modernized and industrialized sectors look very much alike in many occupationally related ways despite their cultural and political differences If that were so, one might expect rather different results in a comparison with provincial France—a difference in mobility consequences (or attitudes) that remains to be shown. It may also be true that the supposed difficulty of upward mobility in France does not pertain to admittance into the white-collar class but only to admittance into the upper reaches of the bourgeoisie—a restricted version of the mobility hypothesis which has certainly not been tested here, is open to doubt, and likewise remains to be shown.

On the attitude side, it may also be true that certain subtle differences in mobility orientation are simply not tapped by the attitude instrument used here;[17] yet, even so, whether these subtleties provoke the status-based consequences discussed in the literature also would remain to be shown. On the other hand, it could be that we have here mainly additional evidence that we need to be more wary about the sundry status assumptions that now characterize sociological work. I don't expect that this one comparative study, limited as it is in so many ways, can or should disabuse us of these assumptions; but it can and should raise further doubts about the ingrained status-mindedness of contemporary American theorizing. That status-mindedness is shown in the still-prominent assumption of the functional necessity of status systems, in the readiness to impute status motives to those who are mobile, in the quick equation of status and ego (an equation on which some recent work concerning black self-esteem casts

[17] I am indebted to the French social psychologist, Dr Claude Faucheux, for his critical reading of an earlier version of the manuscript, and for his inclination to insist upon the presence of such differences in attitude despite the present evidence (e g , differences in status competitiveness and/or in defensiveness rather than risk taking regarding status gain)

considerable doubt [Rosenberg and Simmons 1972]), and, of course, in the assumption concerning a widespread desire for mobility as a first-order priority.

The doubts that are generated concern, most particularly, a greater wariness about the extent to which Western societies, despite their obvious differences in some ways, differ in their status orientations or their mobility consequences; doubts about how readily status changes translate into problems that require special coping mechanisms (i.e., require more than ordinary management); and doubts about the extent to which the mobility effects that do occur cut a wide swath over the person's life space. Seen in this context, the present work is at once a comparative study of mobility and a commentary on the status assumptions that commonly underlie our predictions and our convictions in this domain.

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The Occupational Composition of American Classes: Results from Cluster Analysis[1]

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Hierarchical clustering methods are developed to analyze the American occupational structure. Two analyses are computed, based on similarities of intergenerational mobility inflows and of residential patterns. In both analyses the farm versus nonfarm distinction is the most basic. The second level divides occupations into middle- and working-class clusters. These clusters lend support to a hypothesis of proletarianization of white-collar work; embourgeoisement, manual-nonmanual, middle-mass, and knowledge-technocracy theories are not supported. In both analyses, the majority of clericals and, in the second analysis, technicians are more similar to manual workers than to middle-class occupations. An analysis of friendship choices lends further support to the location of clericals among the working class. The results also document the promise of cluster analysis as a major analytical tool for analyzing social structures.

The analysis of class structure has never been especially amenable to the statistical techniques of American sociology. For the most part, our statistics require interval level or at least ordinal level measures. The general linear model is neatly congruent with the predominant American conception of the stratification system in terms of a continuous, linear hierarchy. The 1947 NORC prestige study (Reiss et al. 1961), the generalization of those results to other occupations by Duncan (1961), and more recent studies by Hodge, Siegel, and Rossi (1964) and Siegel (1971) have provided us with an occupational scale of appropriately detailed gradations.

Other models, such as the Warner system of six discrete bounded classes, the manual-nonmanual dichotomy, not to mention the Marxian and neo-Marxian analyses of class, have been largely displaced in quantitative, empirical research by the continuous scales Most contemporary methodologists' dislike of categorical concepts is well expressed in a recent remark by Bohrnstedt (1974, p. 130): "Most of the variables of interest in the social sciences are continuous and measureable in theory at least at the interval level." So much for Marx.

To be sure, the predominance of continuous models has not been based

[1] I would like to thank two psychologists, Donald Olivier and Larry Jones, for helping me to get started in cluster analysis. Bill Form and Marcus Felson offered helpful comments that clarified the exposition considerably The research reported here was supported by a grant from the research board of the Graduate College of the University of Illinois at Urbana-Champaign.

on statistical convenience alone. There have been cogent and abundant theoretical criticisms directed at class models. Nisbet (1959, p. 14), for instance, is typical in arguing that "the very forces which dissolved the class lines of pre-industrial society acted, in the long run, to prevent any new classes from becoming fixed. . . . The difference between the extremes of wealth and poverty is very great, today as always, but the scale is more *continuous*" (emphasis added). Lenski (1952) has collected data which he interprets as supporting a continuous status model of Americans' perception of the stratification system.

The use of "class" in its original discrete and discontinuous sense has been reserved primarily to European sociologists and Marxian or neo-Marxian analysts. However, among sociologists using class models, there has been little agreement about the composition of those classes in modern society. Indeed, in this perspective, the changing nature of the class structure is the fundamental question about contemporary society.

Embourgeoisement theorists have argued that the traditional distinction between manual and nonmanual labor is breaking down. Affluent, skilled workers are now more similar to nonmanuals than they are to other blue collars in income, job security, home ownership, and social interaction. The embourgeoisement thesis has been disputed by many, most notably in the series by Goldthorpe et al. (1969) They have argued that the economic advances of manual workers have been incorrectly generalized to the conclusion that workers have assumed middle-class life-styles, values, work situations, and patterns of interpersonal relationships. They interpret their data as evidence of a continuing gap in these sectors between the affluent manuals and the lower white collars.

A more extreme rejection of the embourgeoisement thesis argues that white collars are becoming more working class. In American thought, similar perspectives can be traced back at least to Veblen (1921), who argued that the engineers and technicians were in the best position to eliminate the wasteful inefficiencies of the capitalist system. However, Veblen was not especially sanguine about the actual possibilities of engineers combining forces with labor to overthrow capitalism. More recently, Hamilton (1966) has argued that the lower white-collar workers are more working class than middle class.

The proletarianization of white-collar work fits more closely the Marxist prediction of the ultimate conflict between capital and labor. The position of the so-called new middle classes is ambiguous in such a conflict, as Marx fully recognized. The more radical approach to the new middle classes is to argue that due to the bureaucratization and centralization of work their interests have become similar to those of the traditional working class. Gorz (1973, pp. 23–25), for instance, argues: "It is a distinctive characteristic of capitalism to organize the work process in such a way as to pre-

determine the task and competence of each worker, whether blue- or white-collar. The parceling-out of the task, the definition of narrowly specialized responsibilities, the resulting fragmentation of work all . . . keep the responsibility and initiative that are required from him within preset limits so as to allow permanent control from above. . . . This pattern, which has been traditionally applied to manual labor, has been extended to intellectual or white collar labor in such fields as information, communications, administration, research, etc " This more orthodox approach maintains the distinction between capital and labor as the decisive break in modern society.

Dahrendorf (1959), following Geiger (1949), has taken a somewhat different approach, arguing that the ultimate conflict in postcapitalist society will be organized around the distinction between those who possess authority—especially organizational bureaucratic authority—and those who are merely subject to that authority Clerks and technicians would be relegated to the subordinate class in such a model, while corporation executives, though not capitalists in the true sense, would be genuinely a part of the dominant class.

Yet another version of the class structure of modern society has been proffered by Bell (1973), who argues that the growth of social and technological expertise is the characteristic feature of modern society. He notes especially the growth of the "professional technical and kindred" classification and the shift from industrial to service predominance in the economy. From this he argues that future classes will be divided by their relationship to knowledge.

There is an empirical aspect to these questions that is not adequately investigated by existing techniques Hamilton (1966) has tried to demonstrate the similarity of white-collar income to the income of skilled manuals. Glenn and Alston (1968) have attempted to utilize attitude scales to identify social groups. Laumann (1966) has looked for significant differences in perceived social distance between various occupations. All of these attempts have been less than satisfying, partly because they have sought to use an inherently continuous dimension (income, social distance, or attitudes) to develop a categorization of occupational roles. Virtually by definition gaps do not exist along such dimensions—new occupational categories can be introduced or distinctions within the occupational categories can be made to fill in the spaces that do occur.

The recent techniques developed by Thiel (1970) and Goodman (1972*a*, 1972*b*) are an important exception to the dearth of categorical techniques available to the sociologist. Before their introduction, the sociologist working with categorical concepts such as class was forced back on the shopworn cross-tabulations and χ^2. Discriminant-function analysis has been available as a somewhat more powerful multivariate method, but it still

forced the analyst to conceptualize the nominal groups in a multidimensional space that was continuous and linear, not to mention multivariate normal.

The advances introduced by Goodman should make categorical constructs more respectable and available to quantitatively oriented sociologists. But this development only increases the importance of selecting the appropriate categorization scheme in the first place In stratification research, for instance, what categories do constitute the empirical classes of modern society, and how does the composition of these classes shift according to institutional context or national setting?

CLUSTER ANALYSIS METHODS

These questions, and in particular the question of embourgeoisement and proletarianization, can benefit from the recent advances in cluster analysis techniques (Everitt 1974; Sneath and Sokal 1973; Anderberg 1973). Progress in this area has been widespread, especially in biology, phylogenetic taxonomy, and cognitive and social psychology. A recent review article (Bailey 1974) has now made these advances more available to sociologists. White, Boorman, and Breiger (1976; Boorman and White 1976) have applied a clustering algorithm to network analysis. The objectives are basically similar in the research reported here, although the methods developed by White et al. are restricted to data in which "ties" between elements are either present or absent. Most clustering techniques, like the one developed here, utilize quantitative information about the strength of ties between elements.

A variety of cluster analysis methods exists, and since they are thoroughly described in the references mentioned above, there is no need to review them here. All clustering techniques seek to group together elements that are similar and to dissociate ones that are dissimilar. The result is usually a set of clusters, each of which is composed of units which are more or less homogeneous—that is, the units in the cluster are more similar to each other than they are to units outside the cluster, and all similar units are included within the cluster.

The resulting clusters are genuinely nominal classifications. The clusters are not necessarily categories defined along some continuous dimension. That is, for our purposes, they are not necessarily empirical strata, which can be ranked in some hierarchy. They are the empirical analogues of classes (see Dahrendorf 1959, pp. 75–76). Techniques such as multidimensional scaling, factor analysis, and smallest-space analysis are available to construct rankings along continuous dimensions. It should be emphasized, therefore, that cluster analyses are appropriate for a quite different model

of the stratification system—a model based on discontinuous groups with distinct characteristics.

It should also be clear that the technique is not designed to choose between continuous and discontinuous models of the stratification system. It assumes a discontinuous model and then can decide between alternative clusterings. This is the same strategy as the studies which assume a continuous model and then try to find the correct position of an occupation along such a continuous scale. Both methods can answer important questions within their assumptions (e.g., Do government bureaucrats have more or less status than managers in private business? Are clerks in contemporary society more working class than middle class?) But neither can determine which set of assumptions is more empirically useful. Another paper (Vanneman and Pampel 1976) directly compares continuous and discontinuous measures of occupation and shows that even the crude manual-nonmanual dichotomy outperforms the prestige scale in a variety of analyses. Cluster analysis should tell us whether the manual nonmanual classification is optimal or whether some other clustering provides a better description of the divisions in the occupational system.

Hierarchical clustering.—The family of techniques known as hierarchical clustering analyses are especially appropriate to the analysis of stratification systems. The attractive feature of hierarchical schemes is that with these methods each cluster is itself a combination of smaller clusters. A cluster need not be seen as perfectly homogeneous but can be broken down into its constituent groups. In application, this means that while we may wish to make a basic distinction between subordinate and dominant classes, we can still recognize internal differentiation within those classes. This is the common method of conceptualizing class structure. Marx (1951, p. 62), for instance, argues that the French bourgeoisie was divided into the Legitimists (the large landed proprietors) and the Orleanists (the capitalists), with the capitalists themselves divided into the large industrialists and the finance aristocracy. And yet, in its opposition to the proletariat, the bourgeoisie was itself a single class.

Hierarchical clustering is exemplified by the results portrayed in figure 1. The data are from Laumann's (1973) analysis of friendship networks in Detroit, and the hierarchical clustering is based on the frequently used maximum distance or complete linkage method developed by Johnson (1967). The analysis has some important weaknesses not present in the results to be reported later, but the basic objectives of the analysis can be understood from this example. According to these results, the basic division in the urban Detroit area is between manual and nonmanual labor. Within the nonmanual category, groups can be discerned that might be identified as upper middle class, petty bourgeoisie, and lower middle class.

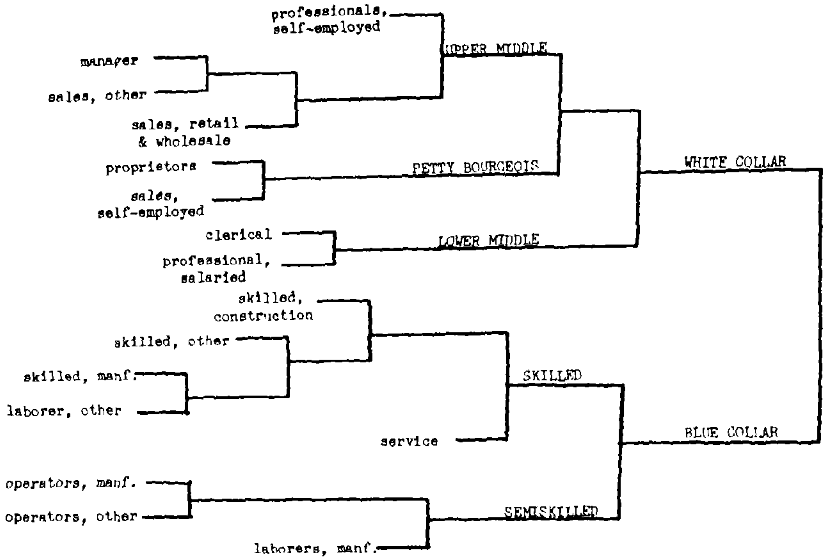


FIG. 1.—Hierarchical clustering analysis (tree diagram) of occupational associations, based on data from Laumann (1973, pp. 75–80)

Hierarchical clustering therefore avoids the question of how many classes exist in a society. This is a false question if we allow for internal differentiation within classes. Since each class is itself a cluster of smaller groups, the number of possible groups the analyst can consider is arbitrary, depending on the fineness of detail the analyst desires. The technique does provide an answer to the following question: If we divide society into two groups or four or fifty, what is the composition of those groups? For instance, figure 1 suggests that if we look at the division into two groups, neither the embourgeoisement nor the proletarianization hypothesis describes the pattern of friendship networks in Detroit. (This inference would be erroneous, however. See the discussion of clustering algorithms which follows.) The method also identifies which division among a set of occupations is most basic and which divisions are secondary. Thus if we were to divide the occupations in figure 1 into upper middle class, petty bourgeoisie, lower middle class, skilled working, and semiskilled working, the most important division is the manual-nonmanual split.

Similarity measures.—The critical operational question in cluster analysis is the definition and measurement of similarity—in this case, the similarity of occupations. It is this measure that determines in which cluster an occupation is placed. Several possible definitions of similarity exist, and there is no reason to expect that the clusterings based on different definitions would be identical. Indeed, the differences in the solutions are im-

portant for understanding the differences in class structure according to the institutional context.

Friendship networks are certainly one important type of similarity that needs to be investigated. Two occupations can be defined as similar to the extent that friendships cross occupational lines; they are dissimilar to the extent that there are few friendships between the occupations. Besides the friendship exchanges, this paper will use residential integration and mobility interchanges to operationalize occupational similarity.

For hierarchical clustering, similarity must be expressed in terms of a statistic—as either a distance or a proximity measure. An appropriate choice for this statistic is crucial for achieving the desired interpretation. For Laumann's data, he reports indexes of dissimilarity (Blau and Duncan 1967, pp. 43–44, 67–69) between all possible pairs of the 16 occupational categories. It is this dissimilarity measure which is used to define within-cluster homogeneity and between-cluster heterogeneity.

Agglomerative versus divisive algorithms —Hierarchical clusterings utilize either agglomerative or divisive algorithms. Agglomerative clustering begins by joining the two most similar elements into a cluster. It continues by adding other elements to this cluster or creating new clusters from other similar elements. Gradually, as the level of similarity required to join elements and clusters together is relaxed, larger and larger clusters are created, until clustering eventually ceases when all elements are joined in a single cluster. Divisive algorithms proceed in the reverse direction. All the elements are sorted into two or more clusters, and these clusters are then broken down further until only the original elements remain.

Agglomerative algorithms enjoy the advantage of computational ease. Most versions can, in fact, be computed manually, although standard computer programs now exist also. The friendship patterns (fig 1) were analyzed with an agglomerative algorithm that is one of the most commonly used methods in the literature—the complete linkage method described by Johnson (1967).

However, for our purposes, agglomerative algorithms in general, and Johnson's complete linkage method in particular, have serious disadvantages. Our primary interest is in the higher-order clusterings—What is the composition of the two, three, or four main classes in American society?—and not in the lower-order clusterings, for example, which lower white-collar jobs cluster together to form a clerical category?[2] Unfortunately, the agglomerative methods provide only an indirect solution of the higher-order clusterings, and there is no guarantee that the higher-order clusterings are in any sense optimal.

[2] See Daufenbach (1973) for an example of an occupational clustering which concentrates more on identifying these smaller clusters

What is preferable is a direct solution of the higher-order clusters with their subsequent division into smaller clusters—in other words, some divisive method. Divisive methods have the disadvantage of being costly in computer time. For instance, to compare all possible sortings of the Census Bureau's 42 occupational categories into two clusters,[3] it would be necessary to generate $2^{41}-1$, or approximately 2.2 trillion, different clusterings. Instead, iterative techniques are used to uncover the optimal clusterings.

We need first to define what we mean by an optimal clustering. The definition used in this article is derived from the theoretical questions we are asking. We want to know the correct placement of such ambiguous groups as clerks and skilled workers. Thus one characteristic of an acceptable solution must be that each occupation is more similar to its own cluster than to the other cluster, that is, the "distance" to its own cluster is less than the "distance" to the alternative cluster. If it were closer to the other cluster, the occupation would be incorrectly assigned. For example, if figure 1 were to be an acceptable solution, we would have to know that the distance from the clerical category to the other nonmanuals is less than the distance from clericals to the manuals.

This definition does not yield unique solutions. Sometimes from the same set of occupations two or three alternative divisions will result, in which every occupation seems to be correctly assigned.[4] A second criterion is therefore necessary—of the various acceptable clusterings discovered, the optimal solution is the one in which the two resulting clusters are maximally distant. In addition to the item-to-cluster distances, the cluster-to-

[3] In the research reported in this article, the simplifying assumption was made that at each step in the analysis, the occupations to be clustered would be divided into two and only two clusters. This was done for partly theoretical and partly practical reasons. Because the concept of class implies opposition (Dahrendorf 1959), clusters of occupations ought to be considered in pairs. In addition, there was no straightforward way to decide between alternative solutions with varying numbers of clusters. The arbitrary designation of two clusters kept the results consistent with the concept of class and simplified the technique. Since the two resultant clusters were each divided into smaller clusters at a later step, solutions with three or more clusters were eventually developed while still identifying the most basic division among the occupations (see n. 4 below).

[4] The existence of multiple solutions is not as damaging to a clear interpretation as might first be expected. If one of the clusters from one acceptable solution is further broken down, the other solutions generally reappear at this lower level. For instance, in the residential patterns, two solutions were uncovered—farm versus nonfarm occupations and farm plus working versus middle. However, in the first case, the nonfarm occupations were divided in the next step into working and middle, and in the second case the farm occupations were divided from the working class in the next step. Thus, the substantive interpretation was clear—three clusters, farm, working, and middle, exist and the only question was whether the working class clustered first with farm occupations or with the middle class. The farm versus nonfarm distinction was judged to be more basic since the distance between these two clusters was 80, while the distance between the middle and the working (plus farm) classes was only 32.

cluster distance is computed and the solution is accepted which has the largest such distance. In other words, what we want is not just to assign occupations "correctly"; we also want the two resulting clusters to be as different from each other as possible.

Distances to clusters.—Cluster analysis requires that we be able to compute a distance between clusters and from an occupation to possible clusters, for example, between manuals and nonmanuals and from clerks to the manual and other nonmanual clusters. This presents a problem when the only data we have are the original distances among the basic elements. For instance, from Laumann's matrix of coefficients we can determine all the distances from the clerks to the eight manual occupations and to the other seven nonmanual occupations separately, but what is needed is the distance from the clerks to the manuals as a whole and to the other nonmanuals as a whole. It is these occupation-to-cluster distances which will determine the placement of clerks.

Again, several alternatives exist for computing distances to clusters Johnson (1967) has proposed a widely accepted solution according to which the distance to any cluster is the maximum distance to any element in the cluster. To return to our example, among the nonmanual occupations, clerks are furthest from the self-employed professionals ($d = .50$, Laumann 1973, p. 78), and among manual occupations they are furthest from the operatives nonmanufacturing ($d = .44$). Thus, by Johnson's maximum distance rule, the clerks are further from other nonmanuals (.50) than from manuals (.44), and they are incorrectly placed in the middle-class category of figure 1 (This is an apt example of the dangers of an agglomerative analysis for drawing conclusions about higher-level clusters. A divisive algorithm based on the maximum distance rule would identify the major division in the friendship networks as clericals and manuals versus other nonmanuals.)

This maximum distance rule for computing element-to-cluster distances has the advantage of yielding solutions that are invariant under any monotonic transformation of the distance measures. However, there are obvious disadvantages. In computing the clerks' distance from manuals, only the distance from one of the manual categories, nonmanufacturing operatives, was used. All the information contained in the distances from skilled workers, laborers, etc. is lost. This has the undesirable effect of making the distances extremely sensitive to the nature of the original elements in the distance matrix, in this case the original 17-category occupational coding.

What would be better is some element-to-cluster distance measure that utilizes all the elements in the cluster and not merely the furthest one. Average distance measures and weighted averages have sometimes been proposed, but a more direct solution is often possible. If the original data were available and not just the element-to-element similarity matrix, it

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What would be better is some element-to-cluster distance measure that utilizes all the elements in the cluster and not merely the furthest one. Average distance measures and weighted averages have sometimes been proposed, but a more direct solution is often possible. If the original data were available and not just the element-to-element similarity matrix, it

would be possible to combine respondents in all manual occupations into a single category and compute one new dissimilarity index between clerks and all manuals in exactly the same way that the reported measures were computed between clerks and, say, skilled construction workers. Similarly, the clerks' distance from all other nonmanuals can be computed directly and the two statistics compared to decide upon the correct placement of clerks. With the mobility and residential data reported in this article, it is possible to return to the original data to compute the element-to-cluster measures directly That is, we will be using a noncombinatorial method (Lance and Williams 1967; Bailey 1974, pp 83–84) in which the cluster-to-cluster distances are not derivable from the old element-to-element distance matrix.

Iterative solution techniques.—The discovery of solutions for such divisive clustering algorithms depends on an iterative technique for reassigning occupations to clusters. Some arbitrary starting point is required, and the distance from each occupation to its own cluster and the other cluster is computed. If the occupation is closer to its own cluster, it is correctly assigned; if it is closer to the other cluster, it is incorrectly assigned Incorrectly assigned occupations are then switched to the opposite clusters. Since after reassignment of some occupations the two clusters are now different, a new set of element-to-cluster distances is computed Again, any incorrectly assigned occupations are switched and new distances based on the new clusters are computed. The process continues until a solution is reached in which all occupations are closer to their own cluster than to the opposite cluster.

Since the iterative process converges sometimes to multiple acceptable solutions, the discovery of the optimal solution depends on a comparison of cluster-to-cluster distances in the full array of acceptable solutions. Again, we are faced with the problem of computing $2^{N-1}-1$ solutions. The most satisfactory compromise is to experiment with as wide a selection of starting points as possible and to observe whether these starting solutions converge to a limited number of acceptable solutions. The computer routines reported in this research utilized, at each level of clustering, at least $N+9$ different starting points, where N is the number of occupational categories in the original data. These starting points were selected according to systematic rules that were thought to maximize the possibility of uncovering new solutions.[5]

[5] The first starting point took a single occupation in one cluster and all other occupations in the second cluster, computed the element-to-cluster distances, reassigned occupations, if necessary, and followed this process until it converged on an acceptable solution The next step began by isolating a different occupation in one cluster, assigned all the remaining occupations to the second cluster, and again followed the iterative procedure until a solution was discovered. This process was repeated with each occupation taking its turn as an initial, single-element cluster This produced N

CLASSES AS DEFINED BY MOBILITY PATTERNS

The use of mobility patterns to determine class boundaries has a long history in sociology. For example, hereditary aristocracies are defined by the lack of mobility between those positions and the mass of the population The points at which there is the least mobility have been construed as indicators of the existence of class boundaries. For example, Blau and Duncan (1967, pp. 58–75) conclude that two "semi-permeable" boundaries exist, one between farm and nonfarm and another between manual and nonmanual occupations. The extent of intergenerational mobility across such boundaries is often considered to be an indicator of the openness or rigidity of the class system.

Nevertheless, the determination of such boundaries has not yet been given a systematic basis. Most conclusions have rested on a cell-by-cell analysis of mobility tables or, at best, inspection of results from some method of multidimensional scaling This is a problem ideally designed for cluster analysis. Hope (1972) has provided a start in this direction, although his algorithm is quite different from the one proposed here.

The data base for these analyses is the well-known OCG study of American males by Blau and Duncan (1967, pp 496–98). Their published tables reported 17 occupational categories Analyses of the intergenerational mobility tables (father's job to present job) are reported in this article.

Defining occupation-to-cluster mobility rates—In computing mobility rates, investigators will frequently compute two types of mobility matrices, an inflow matrix and an outflow matrix, and analyze them separately (e g., Blau and Duncan 1967, chap. 2) A good case can be made that occupations of origin (e.g., father's occupation or first job) should be the criterion of class similarity. Two jobs are in a similar class position to the extent that people with those jobs have had similar backgrounds.

The use of outflow matrices is less convincing The clusters derived from outflow statistics would represent, for example, clusters of the occupations of fathers with sons whose occupations were similar to each other. This might tell us something about the class structure in the father's generation, but rather little about the current class structure In addition, the fathers are not a single age cohort but span several cohorts, and problems of differential fertility across fathers further complicate analysis.

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starting points, where N = number of occupations to be clustered The best of these solutions was then used to define nine new starting points One was a haphazard reassignment of the occupations so that every other occupation was assigned to the opposite cluster The remaining eight starting points began from pairs of occupations which were most similar or most dissimilar as determined by the occupation-to-cluster distances (for example, the two occupations, one from each cluster, that were at the largest distances from their own clusters, i e, the most "atypical" pair) Initial clusterings were then generated from this pair by assigning each occupation to one or the other member of the pair depending on the occupation-to-occupation distances.

The cluster analysis reported in this paper, therefore, will utilize the inflow matrix to compute similarities among occupations and clusters of occupations. Separate analyses based on outflow similarities and a combined inflow-outflow similarity analysis have been computed and are available from the author.

One of the vexing problems of mobility research is the difficulty of expressing the degree of mobility between two categories in a single statistic (Duncan 1966). For cluster analysis this will be crucial since it will become the criterion for placing occupations in one or the other cluster.

Two distances are computed for each occupation; these distances measure the degree of interchange (actually, the lack of interchange) of the occupation with each tentative cluster. The creation of new clusters and subsequent reassignment of occupations is continued until all occupations are "correctly" assigned.

The occupation-to-cluster distances are computed from two 2 × 2 tables similar to those in figure 2. For each table, the mobility rate is computed

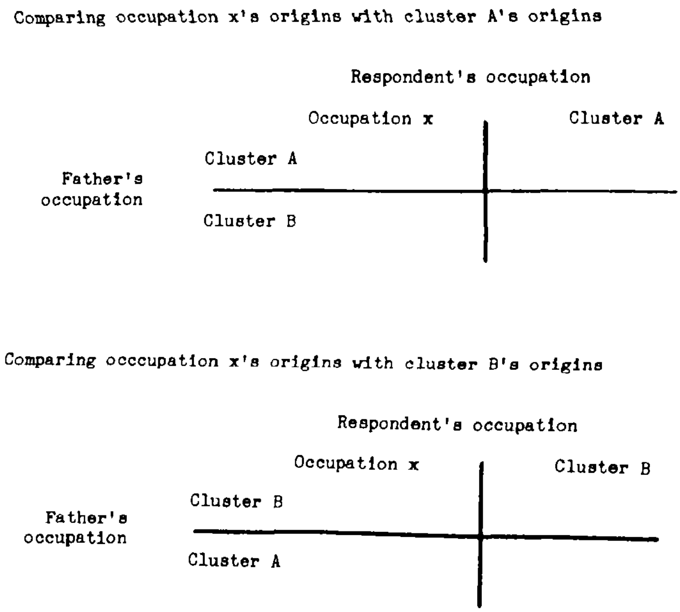


FIG 2 —Occupation-to-cluster distances from mobility tables

nd the occupation is assigned to the cluster with which it has a higher iobility rate. Both tables utilize the proportion of the occupation's re- ›ondents that have cluster A origins as opposed to cluster B origins. This ·oportion is compared with the proportion of cluster A respondents with uster A origins and the proportion of cluster B respondents with cluster A igins. Occupation X "belongs" to cluster A if X's origins are more similar

to A's origins than to B's origins, that is, if the proportion of X's with A origins is closer to the proportion of A's with A origins than to the proportion of B's with A origins.

However, even for 2 × 2 tables, comparisons of mobility rates are hazardous. The statistic to be used depends on the meaning we wish to give to mobility and on how we wish mobility to represent class similarity. Despite the advantages of a single statistic, there is no widely accepted measure for comparing mobility rates in a 2 × 2 table. Simple proportions, ratios of expected to observed frequencies, and indexes of dissimilarity are possibilities used in earlier research. The distance measure used in this analysis has been γ, a well-known measure of association. (Since we will be computing gamma only for 2 × 2 tables, this measure is identical with Yule's Q.) A γ of 0.0 would mean that the origins of the occupation were distributed exactly like the origins of the cluster A high positive γ would indicate that respondents' occupations were closely related to their origins, that is, that cluster A's origins were predominantly cluster A while occupation X's origins were predominantly cluster B The γ is therefore a distance and not a similarity measure, the higher the γ the greater the distance between the occupation and the cluster.[6] The γ has some attractive features for our purposes: (1) It is insensitive to changes in the marginals of the table. If any column or row of the table is multiplied by a constant, γ remains unchanged What this means is that when we are deciding to which cluster an occupation belongs, the relative sizes of the two clusters do not bias the occupation-to-cluster distance measures (2) Distance is at a maximum (1.0) if there is either no inflow from or no outflow into the cluster. While an occupation may give many people into a cluster unless it also receives from the cluster, distance is a maximum This places a premium on equal interchanges. There are advantages to a cluster analysis of the stratification system which groups together the occupations that are optimally equal. Equality, in the case of mobility tables, means both giving and receiving One without the other defines a hierarchical relationship. Clusters composed of occupations linked only by such hierarchical interchanges might reflect situs categories (Hatt 1950), but the bias of γ toward equal interchanges should build clusters that are more analogous to traditional classes.

Results.—Figure 3 reports the results of the cluster analysis of the father's occupation to present occupation matrix. As with all the results to be reported, the most basic division in mobility interchanges is between agricultural and nonagricultural occupations. This should come as no sur-

[6] Negative γ's are possible and not uncommon in this method They indicate that occupation X has an even higher proportion of cluster A origins than does cluster A itself The interpretation is consistent with the above rules the more negative the γ the closer the occupation is to the cluster.

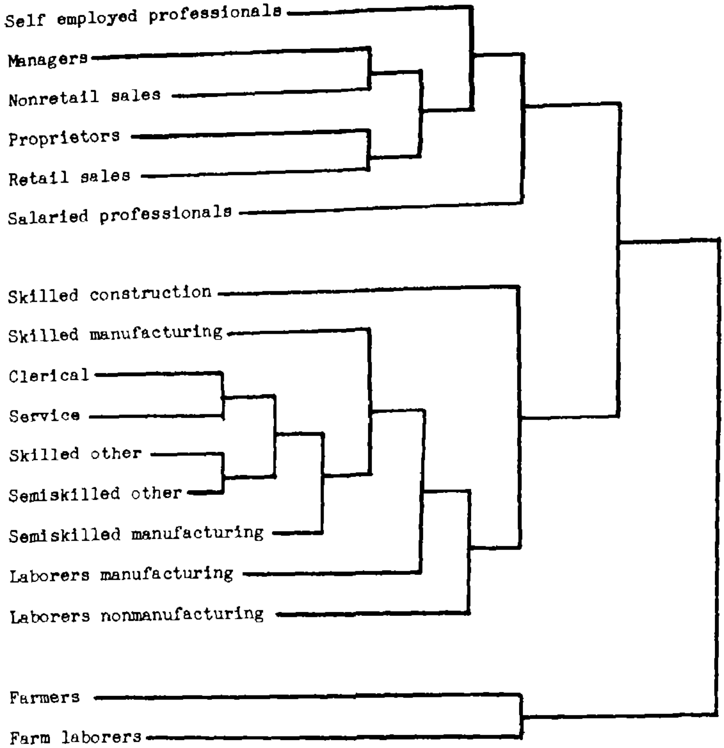


FIG. 3.—Clustering by intergenerational mobility inflows

prise, but it is a useful reminder after many discussions of class structure which implicitly relate only to urban society while neglecting the more fundamental agricultural versus nonagricultural gap.

The nonagricultural occupations are divided into two large groups which might be called "middle" and working classes. The most notable feature of this division is the placement of the nonmanual clericals with all manual workers in the working-class category.

Some 71% of the clerks had blue-collar fathers.[7] This compares with 50% of the nonmanuals and 84% of the manuals who had blue-collar fathers. The clerks' percentage is judged closer to the manuals' 84% than to the nonmanuals' 50%, since the clerk-to-manual distance ($\gamma = .36$) is less than the clerk-to-nonmanual distance ($\gamma = .42$). These distances are reported in columns 1 and 2 of table 1.

Other methods of computing similarity yield the same results. For example, 7.7% of the men with manual fathers became clerks, while only 7.0% of the men with nonmanual fathers became clerks. Thus, by this

[7] These percentages refer only to those clerks who had either manual or nonmanual (excluding clerical) fathers, not to clerks with agricultural fathers See figure 2.

TABLE 1

DISTANCES (γ) TO WORKING- AND MIDDLE-CLASS CLUSTERS MOBILITY DATA

	Distance to Middle Class	Distance to Working Class	Difference
Self-employed professionals	— 31	77	—1.07
Managers	19	54	—0 36
Nonretail sales	— 01	62	—0 63
Proprietors	.32	43	—0 12
Retail sales	13	55	—0.42
Salaried professionals	19	54	—0 36
Skilled (construction)	.51	17	0 34
Skilled (manufacturing)	60	01	0 59
Clerical	42	36	0 07
Service	.64	— 04	0 68
Skilled (other)	56	14	0.42
Semiskilled (other)	63	— 02	0 65
Semiskilled (manufacturing)	60	01	0 59
Laborers (manufacturing)	78	— 32	1 10
Laborers (nonmanufacturing)	70	— 17	0 87

outflow criterion also, the clerks' origins are still slightly more working class than middle class.

The mobility tables, therefore, lend support to the proletarianization hypothesis and provide no evidence of embourgeoisement. Nor do they provide any support for the hypothesis that classes develop according to the relationship to knowledge, as might have been suggested if the two professional categories had formed a cluster.

Admittedly, the original OCG categories are too crude for any definitive test. Undoubtedly, some segments of the clerical category are more middle class than working class. Perhaps, too, segments of the managerial and ownership elite are sufficiently distinctive to have created an elite cluster in contrast to both the middle- and working-class clusters The data are simply not detailed enough to reveal such patterns. Nevertheless, the gross outlines are clear. The proletarianization hypothesis has fared better than any alternative hypothesis about the empirical clustering of occupations into classes.

The internal differentiation of the middle- and working-class clusters is also portrayed in figure 3. These results indicate the "chaining" tendency (Lance and Williams 1967) of many hierarchical clustering algorithms. At each stage except the last the original cluster is divided into a single occupation cluster and a cluster of the remaining occupations. The substantive interpretation of these clusters is of less theoretical significance than the earlier results. In the middle-class cluster, we may note the early differentiation of the two professional categories and the local commercial and business clusters at the last step. Within the working class, the existing OCG categories seem to define their own distinct clusters

These results are similar to Hope's (1972) analysis of the OCG data, despite the fact that Hope's clustering utilized an agglomerative strategy based on a canonical analysis of ϕ coefficients. The main difference in results is Hope's isolation of self-employed professionals as distinct from all other nonfarm occupations. However, Hope's results may be misleading since the salaried professionals are closer to the self-employed professionals than to the large nonfarm cluster to which he has assigned them. The difference may perhaps be attributed to the arbitrary preference in Hope's scheme for merging two clusters of occupations before merging a cluster with a single occupation. Nevertheless, the difference in results is quite small, and Hope's analysis also assigns clerical occupations to the working class.

CLASSES AS DEFINED BY RESIDENTIAL INTEGRATION

Residential segregation is another important criterion for defining occupational differences and thus the composition of classes. The importance of residential patterns is related to the importance of interpersonal networks in determining class position. In his well-known discussion of the French peasantry, Marx cites the isolation of the peasant as inhibiting the development of a politically active class (Marx 1951, p. 156). In a similar vein, we would expect that the growth of class conflicts would proceed more rapidly to the extent that members of a class live in close proximity with one another but at some distance from opposed classes.

Residential patterns have been used in the past (Duncan and Duncan 1955*b*) to define distances between occupations However, the 1950 census provided only a crude 10-category occupational classification, and the Duncans did not have available a suitable method for further analyzing the 10×10 matrix of segregation coefficients.

The 1970 census provides a 42-category occupational classification at the tract level for all Standard Metropolitan Statistical Areas (SMSAs). Results from Peoria, Illinois, will be reported here The Peoria SMSA had a total population of 341,979 in 1970, which made it the 96th largest of 223 SMSAs in the country. Thirty-five percent of the labor force was employed in manufacturing, somewhat higher than the national average of 26%, 45% were nonmanual workers, very close to the national average of 48%.

To control for the overriding importance of race in residential patterns, the analysis was confined to whites who were 16 years of age or older. Unfortunately, the number and distribution of blacks in the Peoria area were insufficient to compute a cluster analysis on blacks separately.

The Gini coefficient of inequality was used as the measure of segregation

between any two occupations or clusters of occupations (Duncan and Duncan 1955*a*). This distance measure assumes a value of 0.0 if two occupations have the same relative distribution across all the tracts in the area and a value of 1.0 if there are no tracts in which members of both occupations reside. It is the most frequently recommended measure of integration since it is thought to be independent of the size of the occupational categories. In spite of these claims (Duncan and Duncan 1955*a*, p. 215), the measure is biased so that smaller categories tend to have higher segregation scores.[8]

Again, a divisive clustering strategy was pursued. For each trial, occupations were assigned to one of the two trial clusters and distances were computed from each occupation to these two clusters.[9] If the occupation was "further" from its own cluster than from the opposite cluster, it was reassigned. This process was repeated until every occupation was assigned "correctly." that is, until the Gini index of segregation with its own cluster was less than the segregation index with the opposite cluster

Results.—As was true for mobility patterns, the first division among the occupations fell between the agricultural and nonagricultural groups. Not surprisingly for residential patterns, this division was far greater than for any divisions within the two resulting clusters. Eighteen of the 78 tracts in the Peoria SMSA are substantially rural. Because of this basic rural-urban difference, the remaining analysis was based on the urban tracts only.

[8] The problem was recognized when segregation indexes were computed for each occupation separately (i e , between each occupation and all the remaining occupations) The segregation indexes correlated — 76 (Spearman ρ) with the number of people in the occupational category The problem can be understood thus suppose we were to divide any occupational category into two categories, randomly assigning individuals to one of the subcategories If each census tract received the same proportion of each subcategory, the segregation index between a subcategory and an outside category would be equal to the index between the overall category and an outside category In other situations the expected values of the indexes for the subcategories would be greater than the observed values for the overall category This is because when the Gini indexes are computed, the tracts are arranged in order of the percentage of the subcategories in each tract This arrangement will probably not be the same for the two artificial subcategories as for the original major category, thus tending to maximize the Gini coefficients of the subcategories In effect, the coefficient takes advantage of random variation and this random variation has a more pronounced effect on the smaller occupation categories

[9] Because of the bias of the Gini index toward small categories, multiple solutions arose that isolated a series of the small occupational categories as single element clusters (i e , outliers, Bailey 1974, pp 81–82) rather than dividing the occupations into large classes To compensate for this bias in the similarity measure, distances between an occupation and the two trial clusters were computed as if the occupation were already a part of the trial cluster. That is, in every tract, the number of workers in an occupation was added to each of the two trial clusters and the distance was then computed between the occupation and these two trial clusters This builds into the measure a kind of self-correlation, but the effect is much greater on an otherwise small trial cluster than on a big trial cluster.

Figure 4 reports the results. Again the second-level division creates working- and middle-class clusters. The working-class group includes three nonmanual categories—technicians, bookkeepers, and other clerical. One service occupation is included in the middle class—personal service workers,

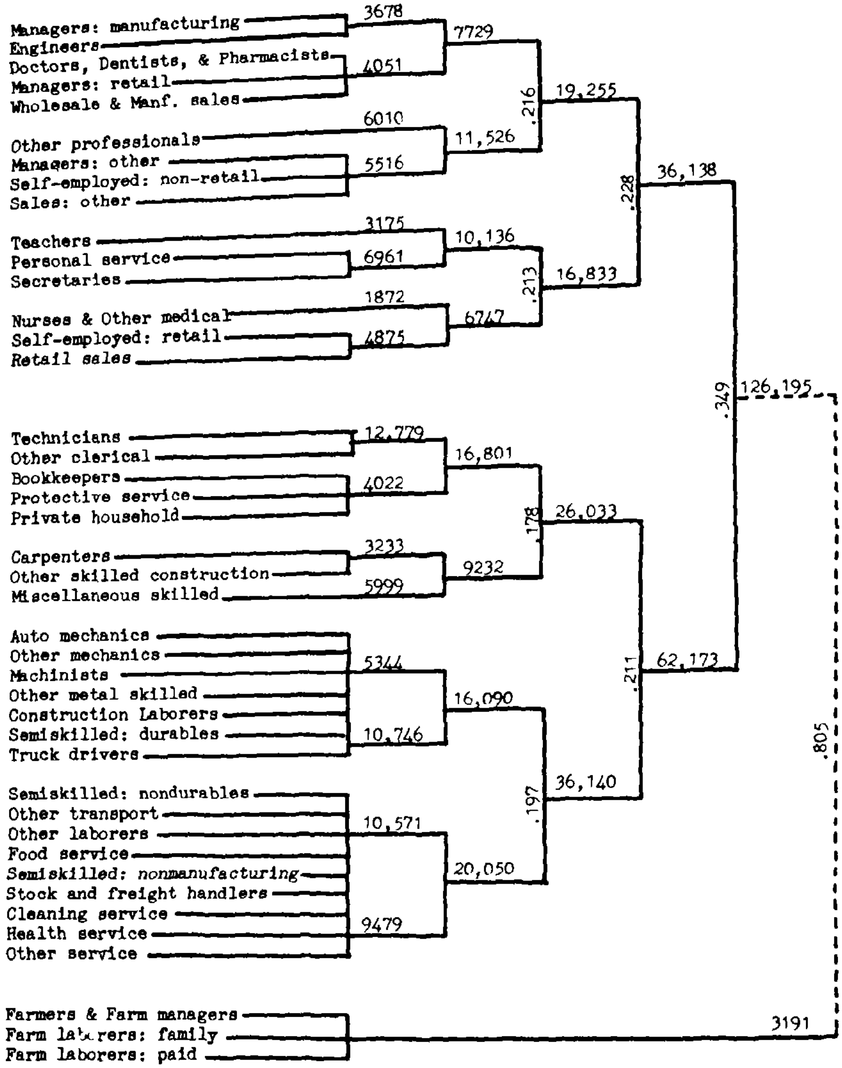


FIG 4.—Clustering by residential integration Peoria SMSA Horizontal figures indicate the number of people in each cluster, vertical figures indicate the segregation coefficient between the two clusters The farm-nonfarm cluster was computed for the entire SMSA All the remaining clustering was computed only for the urbanized portion of the SMSA. This explains why the total for the nonfarm cluster is larger than the sum of the totals for the working- and middle-class clusters.

a small, predominantly female category, 65% of which is comprised of hairdressers and barbers.

However, none of the so-called affluent worker categories reveal residence patterns more similar to the middle class than to the working class. In fact, a large number of supposedly lower-middle-class groups live closer to the working class than to the middle class. All clerical workers with the exception of secretaries fall in the working-class cluster In addition, technicians,[10] classified by the census with professionals, are more working class than middle class in residence patterns. The three nonmanual working-class categories include about 29% of nonmanual workers.

Table 2 reports the distances from each of the 39 urban occupations to the two clusters. Comparisons across occupations in column 2 or 3 would be misleading since the distance measure is biased by the size of the occupational category However, it should be noted that the lower nonmanual occupations and the self-employed categories are close to being equidistant from the working-class and middle-class clusters The support for the proletarianization hypothesis is, therefore, weak, but that hypothesis is nevertheless the best explanation The skilled manuals are quite clearly working class in this clustering The data provide no support for the embourgeoisement hypothesis.

Figure 4 also reports some of the internal differentiation within the middle and working classes. The middle class is divided into an upper, predominantly male cluster and a lower, predominantly female cluster. The division in the working class is less easily interpreted. The upper group contains all the nonmanuals and about two-thirds of the skilled workers; the lower group contains the remaining skilled workers, all the semiskilled laborers, and the unskilled service occupations.

However, the integration among the various segments of the working and middle classes is much greater than between the two classes themselves. The segregation coefficient for the middle versus working classes is .349, while the division of these two classes yields coefficients of only 228 and .211. This suggests that the dichotomous model, besides being parsimonious, describes the basic division in urban society.

Some question might arise that the placement of the clerical workers and technicians may be affected by the large number of wives of manual workers who fill these types of jobs. The residence patterns of these women might be more working class only because they tend to be married to manual workers, and it is the occupation of the male head of household that determines the residence.

[10] This category is composed primarily of draftsmen (44%), engineering and science technicians (42%), radio operators (5%), and surveyors (4%)

TABLE 2

DISTANCES (γ) TO MIDDLE- AND WORKING-CLASS CLUSTERS. RESIDENTIAL SEGREGATION

	Total Sample				Male Estimates			
	Total	Middle Class	Working Class	Difference	Total	Middle Class	Working Class	Difference
Middle class								
Managers (manufacturing)	1,609	28	52	— 24	1,582	.22	52	— 29
Engineers	2,069	32	53	— 21	2,069	27	53	—.26
Doctors, dentists, and pharmacists	659	47	68	— 20	625	45	68	— 24
Managers (retail)	1,481	26	42	— 16	1,150	31	46	— 15
Wholesale and manufacturing sales	1,911	28	51	— 22	1,830	25	51	—.26
Other professionals	6,010	14	39	— 24	4,182	15	41	— 26
Managers (other)	3,418	.18	41	— 23	3,005	16	43	— 26
Self-employed (nonretail)	434	.42	43	— 11	382	.50	.59	— 09
Sales (other)	1,664	23	44	— 21	1,213	29	50	— 21
Teachers	3,175	21	38	— 18	968	35	45	— 10
Personal service	1,490	32	33	— 00	*	*	*	*
Secretaries	5,471	.17	25	— 08	†	†	†	†
Nurses and other medical	1,872	25	40	—.15	‡	‡	‡	‡
Self-employed (retail)	566	42	44	— 01	434	.53	48	05§
Retail sales	4,309	22	26	—.04	1,576	.29	38	— 09
Working class.								
Technicians	1,121	36	34	01	1,013	39	.36	03
Other clerical	1,658	20	15	05	5,106†	27	22	.06
Bookkeepers	2,270	.27	24	.03	†	†	†	†
Protective service	1,016	.48	43	.05	944	52	.43	09
Private household	736	.47	44	02	*	*	*	*
Carpenters	768	.43	39	05	753	.46	.39	.08
Other skilled construction	2,465	35	21	13	2,439	.39	.21	18

TABLE 2 (*Continued*)

	Total Sample				Male Estimates			
	Total	Middle Class	Working Class	Difference	Total	Middle Class	Working Class	Difference
Miscellaneous skilled	5,999	27	17	10	5,547	.30	17	.13
Auto mechanics	829	49	34	14	812	.53	.35	19
Other mechanics	1,705	40	25	15	1,693	44	25	19
Machinists	834	49	33	16	824	53	.32	.22
Other metal skilled	1,434	44	28	16	1,403	.48	29	.20
Construction laborers	542	59	39	20	540	.63	38	.25
Semiskilled (durables)	9,034	39	20	19	8,499	38	.19	.19
Truck drivers	1,712	48	26	22	1,694	51	25	26
Semiskilled (nondurables)	2,572	44	25	19	1,654	.55	35	.21
Other transport	2,248	42	21	21	2,145	.47	25	23
Other laborers	1,897	48	32	16	1,799	52	.33	.19
Food service	3,854	37	20	18	1,091	44	39	05
Semiskilled (nonmanufacturing)	2,876	40	19	21	1,802	45	23	22
Stock and freight handlers	2,068	43	21	23	1,897	47	.22	.26
Cleaning service	2,378	43	24	19	1,683	49	.28	21
Health service	1,478	49	33	16	*	*	*	*
Other service	679	55	44	12	654*	55	.42	12

* The private household service, health service, and other service categories of the total sample were combined into a single category, other service, for the male estimates

† The secretaries, bookkeepers, and other clerical categories of the total sample were combined into a single category, other clerical, for the male estimates.

‡ Nurses and other medical were combined into the technicians category for the male estimates

§ In this analysis of the male estimates, the self-employed retail category clustered with the working-class category

Unfortunately, the census tract data do not report an occupational breakdown of heads of household. However, a close estimate[11] for the distribution of occupations among males can be made, and this will provide a rough test of the alternative hypothesis.

Thirty-four male nonagricultural occupational categories can be derived from the census data. These categories were analyzed using the same procedures as for the total population. The results are substantially the same. Male technicians and clerical workers remain in the working-class cluster while none of the skilled manuals fall in the middle class. The only difference for these data is that self-employed retail managers (store owners) are slightly closer to the working class than to the middle class. Of perhaps more interest, the distance between the middle and working classes appears much greater for the men (0.431 as compared with 0.349 for males and females combined). This suggests that the division into classes may be more pronounced for men than women.

DISCUSSION

The objective of this paper has been twofold: to provide an empirical test of an important substantive question in the stratification literature and to introduce some new developments in cluster analysis methods

The pattern of the results is convincing. Utilizing two different operational definitions of similarity, two different statistical measures of association, two different categorization schemes, and two different geographic bases, the analysis has supported a single conclusion. The lower white-collars are clustered with the working class as predicted by the proletarianization hypothesis. Embourgeoisement, middle-mass, manual-nonmanual, and knowledge-technocracy predictions are not supported.

This is further supported by the analysis of the friendship choices reported by Laumann. Although without the original data it was necessary to rely on a maximum distance rule based on the original occupation-to-occupation distance matrix, a divisive cluster analysis again identifies the manual-plus-clerical versus nonmanual split as the basic division in the American occupational structure.

The results are more ambiguous about the internal stratification of the middle and working classes (Form 1973). There is no clear skilled versus

[11] The census does not report a separate occupational distribution of males by census tract A female distribution is reported and where these categories mirror the categories for the total population, the male distribution can be obtained by subtraction. However, some female categories are composites of several total categories For these categories, estimates of the male distributions in each tract were created by subtracting a fixed percentage of the more general female category (e g., skilled) from the more specific total population category (e g, skilled manufacturing) These fixed percentages were determined by the detailed occupational distributions available for the entire Peoria SMSA as reported in printed census reports.

nonskilled split in either analysis. Nor do the mobility and residential segregation results yield similar divisions in the working class. Among the middle class, the lower-middle-class grouping found in the residential clusters is not replicated in the mobility data. The one consistent interpretation that can be drawn from these results is that the divisions within the working and middle classes are less pronounced than the division between them.

Of course, the results are far from definitive, and one could hardly pretend that even the empirical aspects of these questions have been resolved. The data bases need to be expanded to more recent time periods with national or even cross-national samples. More detailed categorization schemes can be employed For instance, elite groups need more thorough identification and inclusion in the analysis Similarity can be defined in other ways—for instance, from investigations of friendship and communication networks, intragenerational mobility, the content of interpersonal interaction, and perceived similarity judgments.

These problems for future research are clearly defined and manageable. The present results suggest the possibilities of the method as a new tool for investigating social structures While sociologists' ability to study processes and causal models has increased dramatically in the past two decades, the mathematical analysis of structures has only begun to develop. Cluster analysis, particularly hierarchical methods, holds great promise in this effort.

Beyond these technical difficulties, we need to consider more carefully the limitations of cluster analyses in the debate about the class structure of modern society. Certainly the clusters reported in this paper are not classes in the traditional sense of that construct, that is, the clusters are empirical groupings and not analytical classes. That distinction is important to maintain. None of this research has, or could have, established the divisions around which interests are organized into conflicting groups.

Furthermore, the clusters identified in this research reflect a static picture of the social structure. This is especially important in considering their relevance to embourgeoisement or proletarianization hypotheses which are inherently dynamic. The placement of technicians and clerical workers with the working class says nothing about whether they are now more similar to the working class than in the past. The question of change can be answered better with other techniques, although cluster analysis should be used to identify the basic groupings to be investigated. With longitudinal data we can then see whether these groupings are becoming "looser" or "tighter" or whether they are shifting in composition.

This analysis is not dynamic in the sense also that the causal determinants for the various clusterings are not identified. We do not know whether technicians and clerks are grouped residentially with the working

class because of their incomes, family origins, or occupational histories. Similarly, the linkages between fathers' and sons' occupations are not revealed by the clustering, nor is the role of the growth of nonmanual employment clear. These questions could be investigated with more detailed data on which to base the clusterings. Do upper-income technicians reside with the middle class and lower-income technicians with the working class? Do the clusters that result from an analysis of a joint income and occupation categorization reflect income more closely than occupation?

Nevertheless, the results of the cluster analyses reported here are far from irrelevant to the understanding of the phenomenon of class. To the extent that the clusters revealed by the data are coincident with analytical classes, we may conclude that the process of class formation from *Klasse an sich* toward *Klasse fur sich* is more likely. The inclusion of 20%–25% of the nonmanual workers in the American working class is further than most empirical arguments have been willing to venture (but see, for example, Hamilton 1966). While some analytical approaches have argued that much of nonmanual labor should be correctly placed in the working class, the empirical realities have not generally been assumed to have progressed that far or even in that direction. The results reported here suggest a substantially larger, more coherent working class than the earlier, less systematic observations revealed.

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Problematics in Stratum Consistency and Stratum Formation: An Australian Example[1]

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Questions relating to the consistency of the stratification order can be posed at the level of social aggregates as well as at the individual level. This paper draws on Australian socioeconomic data to illustrate issues involved in defining relatively homogeneous social strata in terms of commonly occurring status profiles and in exploring the effects of stratum characteristics such as degree of stratum homogeneity and stratum attribute consistency. Positional awareness, defined in terms of class identification and voting behavior, is examined in relation to the contextual variables of stratum homogeneity and stratum attribute consistency, which show weak effects once the average level of socioeconomic status of different strata is taken into account. The low salience of such contextual variables is explained partly by relatively high stratum permeability, reflected in high rates of father-to-son mobility, and partly by the difficulty of establishing that the strata as defined are perceived as naturally occurring groups.

The topic of status consistency (and its converse) has proved to be of enduring interest to sociologists. Substantively, status consistency research addresses questions relating to the following kinds of theories: functional theories of stratification that typically hypothesize equilibrating processes tending to align an individual's position on different scales of qualification, reward, and evaluation (see Wesołowski and Słomzynski 1974) and theories of role conflict and cognitive dissonance.

STATUS CONSISTENCY VERSUS STRATUM CONSISTENCY

Questions relating to status consistency can be posed at two different levels: that of individuals and that of the stratification order. Research on status consistency has concentrated on equilibration at the individual level and on the presumed stress experienced by people who simultaneously occupy social roles that command unequal rewards, whether material or symbolic. For general reviews, see Jackson and Curtis (1972); Segal, Segal, and Knoke (1970), and Segal and Knoke (1971). The outcome of this research is conflicting, partly because it is largely made up of secondary analyses of survey data collected for other purposes and partly because rather

[1] Revision of a paper presented at the Eighth World Congress of Sociology, Toronto, 1974.

strong (and largely untested) assumptions are required about stress-producing and stress-reducing behaviors (Broom and Jones 1970, pp. 999–1000). It has not been satisfactorily demonstrated whether status inconsistency is perceived and experienced as stressful by the person or exists merely as a construct in the eye of the researcher.

Irrespective of the unresolved methodological issues on status inconsistency studies at the interpersonal level, it is surprising that so little attention has been given to consistency at the level of social aggregates (for an exception, see Landecker 1963) or to the process of stratum formation. It may be that the research emphasis on mobility has distracted attention from the investigation of the formation of strata and classes. In this paper we attempt to redress the balance a fraction by focusing on the consistency and inconsistency of social aggregates, as manifested in the similarity of the social profiles of their members We are not primarily concerned with how far individual profiles are internally consistent, although, where the several dimensions of stratification do partially coalesce into a unifying scheme, a single, ordered set of strata would be more likely to emerge. "If a stratification system is made up of strata whose populations are homogeneous, and if the strata are composed of individuals with flat status profiles, one might expect the individuals readily to perceive the strata as distinctive entities and to be aware of their own positions in the hierarchy of strata. Such conditions would contribute to the transformation of a statistically distinguishable stratum into a consciously identifiable class" (Broom 1959, p 434) But to emphasize the central point: our major interest is an attempt to discern social strata composed of persons with similar status profiles, and we make no a priori assumption that the individual profiles are consistent or inconsistent, as conventionally defined in most of the status inconsistency literature. It is worth observing that processes of structural change such as immigration and mobility from farms into cities may create large groups of persons with inconsistent statuses (e.g., low educational attainment combined with higher occupational status), groups which may persist as broad strata for relatively long periods of time. It is also a common observation that the occupational experience of some ethnic and racial groups may be inferior to their educational attainment, so that it would be unduly restricting to insist that social strata must consist only of status-consistent persons.

In an attempt to come to grips with these issues in a concrete way, we draw on data from a national sample survey of Australia conducted in 1965 (Broom and Jones 1969) and examine the distribution of socioeconomic profiles defined in terms of educational attainment, present job, and amount of income among employed Australian men. From these data we identify commonly occurring status profiles and aggregate like with like into relatively homogeneous strata. In aggregating profiles, we do *not* as-

sume that they are composed of status-consistent individuals. On the contrary, we allow for the possibility that significant segments of a population may be characterized by status inconsistency. It cannot be presupposed that strata must be consistent, that is, made up of persons with flat (equilibrated) status profiles. It can only be required that strata are reasonably homogeneous in their membership, whether or not their members have consistent profiles.

The hypothetical example outlined in figure 1 makes the foregoing distinctions more specific. With only a simple model of social differentia-

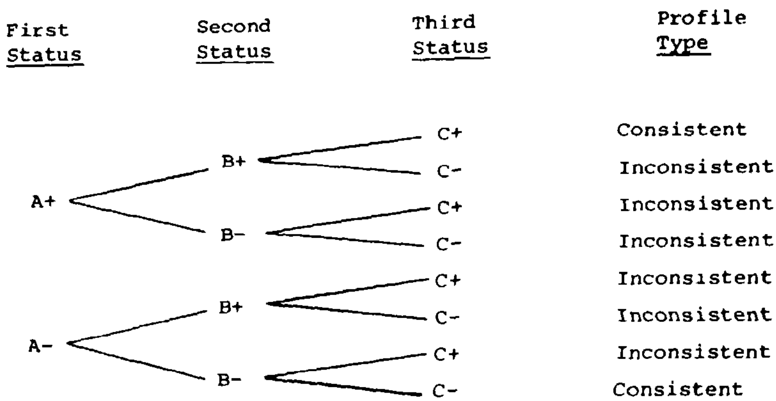


FIG. 1

tion involving three different statuses, each scored high or low, eight different status profiles can be produced. Of these eight only two are fully consistent. It is not difficult to think of instances in which the inconsistent profiles might be compatible with the formation of social strata, considered as groups of persons with similar life chances and life experiences: some jobs with high income do not always require high education (e.g., high-risk jobs and entrepreneurship). Or jobs with high status and high income may require high educational entry requirements but may also screen for racial-ethnic background. The latter process would produce job status If it persisted over long periods of time, social strata composed groups of people whose high education was not reflected in their income or of persons with inconsistent statuses might emerge. Our analysis allows for that possibility. If we were to restrict our search to consistent centers only, we would be forced to consign inconsistent centers to residual and, presumably, evanescent categories. Yet if they are the product of sustained social processes, such an interpretation would clearly be wrong.

It is important to distinguish not only between status consistency and stratum consistency but also between stratum attribute consistency and stratum homogeneity. In the remainder of this paper, we shall use the

term "stratum homogeneity" to refer to the similarity of status profiles within a stratum, that is, as a measure of within-stratum variability of profiles without reference to their consistency at the individual level. Stratum attribute consistency will be used in the sense suggested by Broom (1959), as a measure of the extent to which a stratum's overall profile approaches equilibrium: a stratum is consistent if its profile over the relevant set of status attributes is located at similar points in those different hierarchies. The operational definitions of both concepts are given below. Note that, since in our terminology only groups with relatively homogeneous status profiles can usefully be termed strata, heterogeneous categories are left as residuals. The concept of stratum attribute consistency is applicable only to homogeneous strata.

A stratification order with numerous persons scattered through residual categories has different implications from one in which almost everyone is located in one homogeneous stratum or another. Class consciousness and class action are more readily mobilized in the latter type of regime Furthermore, homogeneous strata with flat profiles (high stratum attribute consistency) have the highest latency of all for class-interest perception and class action.

The empirical analysis presented here was foreshadowed in a preliminary account of the 1965 survey (Broom, Jones, and Zubrzycki 1968). In this paper, however, we use an index of general status based entirely on three socioeconomic measures: education, occupation, and income. Our earlier Index of Social Rank also included data on self-assessed class and interviewer assessment of economic position. Since one of the objects of the present analysis is to treat self-assessed class as a contingent property of social strata, we need to exclude it from consideration in our initial clustering. Our primary objective in this paper is to display an approach to delineating a stratification order without assuming that social hierarchies are consistent. That is, we are interested in identifying homogeneous strata rather than forcing disparate types into so-called strata by a process of statistical homogenization. We shall use our data set to determine how far it is possible to discern distinct social strata made up of persons with similar status profiles (regardless of whether those profiles are themselves consistent or inconsistent) and to what extent the occupants of such strata exhibit class consciousness (indirectly indexed by class identification and voting behavior) and closure in recruitment from generation to generation.

IDENTIFYING SOCIAL STRATA

There are at least three important preconditions to the emergence of identifiable social strata in a society: (1) social differentiation and a division of labor in which the incumbents of different social roles receive

unequal amounts of material and nonmaterial rewards, (2) a degree of exclusiveness in patterns of social participation among persons who occupy social roles commanding similar rewards, and (3) unequal opportunities for those incumbents to transmit advantages to successive generations. In a society in which only the first condition were met, the social structure would exhibit inequality with weakly defined social strata. If both the first and second conditions were satisfied, a society would be both unequal and stratified into mutually exclusive social groups hierarchically arranged. In a society in which all three conditions were present, much the same pattern of inequality and stratum membership would be maintained from one generation to the next. In the last case, one would also expect a high degree of social awareness about stratification, although not necessarily acute feelings of deprivation and rivalry, because some inequalities might be perceived as just or at least as part of an accepted social order.

There is no generally accepted methodology for defining social strata in industrial societies. Self-placement involves a degree of arbitrariness, inasmuch as the set of alternative classes offered may not be relevant to the respondent's conceptions, while the reputational and observational techniques appropriate for determining membership of defined social strata in small-scale communities (e.g. Warner, Meeker, and Eels 1949; Hollingshead and Redlich 1958) cannot be readily applied to large population centers, much less national societies, except perhaps for the purpose of identifying a national elite. To delineate the broad structure of a national society, the sociologist must use relatively abstract procedures to discern regularities in the distribution of unequally valued and unequally rewarded social positions.

Our methodology for identifying social strata most nearly resembles the work of a research group (Machonin 1970) which attempted an objective and exhaustive typology of social strata in Czechoslovakia. The Czech study used five indicators of a person's position in the stratification order: complexity of work, life-style, education, participation in management, and income, all categorized in terms of six-point scales. Although these scales yielded a possible total of 7,776 different status profiles, the sample (over 10,000 heads of households) could be exhaustively classified into 1,769 patterns, because many combinations of disparate statuses did not exist. Two methods for defining strata were used, the first based on "consistent centers" and the second on "optimum centers." The first method, as Machonin (1969, p. 732) recognizes, suffers from the deficiency we have already discussed: it rules out the possibility of finding "inconsistent profile" strata. The second, which clustered profiles according to their overall similarity rather than their consistency, yielded more meaningful results, and from it four relatively consistent and three relatively

inconsistent strata were identified. Evidence of stratum consciousness was also found.

We mention this ambitious and extensive study mainly because its general goals and research strategy parallel our own purpose, which is to compare status profiles across three indicators of socioeconomic differentiation in order to identify clusters of individuals with similar status profiles. In the Australian case, each scale (education, occupation, and income) has six categories, so that the maximum possible number of different profiles is 216. Of these, 170 (79%) actually occurred, although few appeared with any frequency: 24 profiles with 20 or more occupants accounted for 57% of our sample. Using techniques of numerical taxonomy and discriminant analysis, we grouped these different profiles into 10 clusters, according to the degree of similarity between status profiles.[2] As we show below, four broad types of status configurations account for most of the status variation in our sample. Table 1 summarizes the characteristics of each stratum. Although we have attached a characterizing label to each stratum, we should warn the reader that the terms are only indicative. To name a cluster is to designate a constructed type and not to exhaust its descriptive variability We also remind the reader what our analysis can and cannot achieve. Only very large representative samples can capture the very top or the very bottom of the stratification regime. We do not expect to uncover either an elite or the substratum of extreme poverty; there are no admitted millionaires or identified Aborigines in our sample. Between these extremes, however, we believe we can broadly delineate the main structural tendencies in the system of socioeconomic differentiation in Australia.

THE STRATA

Tables 1 and 2 list the 10 strata in roughly descending social rank from higher to lower socioeconomic status. Strata 5 and 6 and perhaps also

[2] We are indebted to Paul Duncan-Jones for the computations in this analysis Since initially all possible pairs have to be considered, the whole sample could not be included in the first clustering With 1,921 persons, initially almost 4 million dissimilarity coefficients need to be calculated, a formidable task for only the first step in a numerical taxonomy, even with a high-speed computer Accordingly, two independent taxonomies were performed on two subsamples (each one-third of the sample), yielding 10 clusters in each subsample Of these 10, eight were virtually identical in terms of their socioeconomic profiles. These eight were amalgamated, and the two unmatched clusters from each solution were retained, yielding 12 in all A discriminant function was found for each cluster, and the remaining one-third of the sample was allocated to one of these 12 clusters In a final step, the 12 clusters were further reduced to the 10 shown in the present analysis The measure of dispersion was squared Euclidean distance, and centroid sorting was used For a more extended discussion of the general technique, see Jones (1969, 1972)

TABLE 1

Social Characteristics of 10 Strata, 1965 (Row Percentages)

Stratum	I	II	III	IV	V	VI	*N*
			Social Characteristic				
1							
Education	0	0	0	0	11	89	126
Occupation	0	0	1	3	19	77	126
Income	0	0	0	13	30	57	126
2							
Education	0	0	0	0	32	68	81
Occupation	2	1	9	17	24	47	81
Income	2	20	46	24	5	4	81
3							
Education	6	17	51	26	0	0	181
Occupation	0	0	2	4	61	33	181
Income	0	0	0	7	21	72	181
4							
Education	8	32	60	0	0	0	222
Occupation	0	4	4	15	61	15	222
Income	0	7	35	43	10	4	222
5							
Education	0	0	0	85	15	0	85
Occupation	0	4	13	44	29	11	85
Income	0	13	47	29	11	0	85
6							
Education	0	0	0	68	25	7	128
Occupation	12	31	38	18	1	0	128
Income	6	54	34	6	0	0	128
7							
Education	1	8	91	0	0	0	278
Occupation	4	11	50	23	13	0	278
Income	0	13	59	22	4	2	278
8							
Education	4	40	57	0	0	0	357
Occupation	0	16	53	18	12	1	357
Income	7	70	22	1	0	0	357
9							
Education	0	0	93	6	1	0	218
Occupation	49	48	3	0	0	0	218
Income	13	79	8	0	0	0	218
10							
Education	31	69	0	0	0	0	245
Occupation	44	47	9	0	0	0	245
Income	21	71	9	0	0	0	245
Total							
Education	6	23	46	12	4	9	1,921
Occupation	13	19	23	13	21	12	1,921
Income	6	39	25	12	6	12	1,921

Note.—The six categories for each social characteristic are as follows Education (I) some primary, (II) completed primary, (III) some secondary, (IV) completed secondary, (V) some tertiary, (VI) completed tertiary Occupation (I) unskilled, (II) semiskilled, (III) skilled, (IV) clerical, (V) managerial, (VI) professional. Income (I) <$1,800, (II) $1,800–$2,599, (III) $2,600–$3,399, (IV) $3,400–$4,199, (V) $4,200–$4,999, and $5,000+.

TABLE 2

AVERAGE STATUS LEVELS AND STATUS CONSISTENCY OF 10 STRATA

STRATUM	AVERAGE STATUS*				Stratum Homo-geneity†	Stratum Attribute Consis-tency‡
	Educa-tion	Occupa-tion	Income	General Status		
1. Upper middle class A	5.89	5.72	5.44	5.68	81.7	0.16
2. Upper middle class B	5.68	4.99	3.40	4.69	71.6	0.86
3. Old middle class A	2.98	5.26	5.66	4.63	84.5	1.10
4. Old middle class B	2.52	4.79	3.70	3.67	74.8	0.77
5. Marginal ? A	4.15	4.31	3.38	3.95	75.3	...§
6. Marginal ? B	4.39	2.63	2.41	3.14	73.4	...§
7. Middle mass A	2.90	3.31	3.22	3.14	77.3	0.16
8. Middle mass B	2.53	3.28	2.17	2.66	71.1	0.41
9. Working class A	3.07	1.55	1.94	2.19	98.2	0.59
10. Working class B	1.69	1.64	1.88	1.74	95.1	0.10

* The average scores are calculated by giving a score of 6 to the highest category, 5 to the second highest, and so on down to 1 to the lowest. General status is the weighted average of the scores for education, occupation, and income.

† Stratum homogeneity is defined as the percentage of profiles which differ by $\sqrt{2}$ or less from the most common profile in each stratum. See text.

‡ Stratum attribute consistency is defined as the mean of the sum of the absolute differences of each status indicator from general status of the stratum. The lower the average deviation, the higher the consistency. Because strata 5 and 6 seem to be residual categories, we have not calculated stratum-attribute-consistency figures for them. See text.

§ Not applicable.

stratum 2 seem in some respects to stand outside the main ordering and to constitute a residual heterogeneous category. For that reason, we have not treated strata 5 and 6 as being strata in the strict sense. even though, by comparison with some other strata, their internal profiles are not especially heterogeneous.

The first stratum is consistently high status, in the stratum attribute sense. Its members are predominantly in the higher-income brackets, hold professional or managerial jobs, and have experienced higher education. The central profile in this stratum is a man with tertiary education, a professional job, and a yearly income of $5,000 or more (the highest-income group identified and the top 10% of male income earners). Of the men in this stratum, 82% had profiles which differed by no more than one or two ranks from this central pattern (see table 2).[3]

Stratum 2 is similar to the first, in that it consists entirely of tertiary-educated men (although fewer with completed tertiary education) who come from a wider range of occupations and earn significantly lower

[3] As a measure of profile consistency, we have used a difference of $\sqrt{2}$ or less in status profile. To fall within this range of variation, a man might have a profile that varied by only one rank on two of the three statuses. A variation greater than one rank on any status or a variation on all statuses falls outside this range. Machonin (1970, p. 734) used $\sqrt{6}$ for five statuses. This allows a slightly wider range of variation, since a difference of two ranks on one scale might be accepted if the other statuses were near identical.

incomes. To some extent, the last is a life-cycle difference. In this stratum, 44% of the men are under 35 and thus at an earlier stage of their careers than men in the highest stratum, of whom only 24% are under 35. Some of these younger men would probably move into higher salary brackets later in their careers. Stratum 2 is in some ways an image of stratum 1 at an earlier stage in the life cycle. We cannot be certain that the differences between the two top strata would be reduced if we had lifetime profiles for both groups, but, because we suspect that is the case, we have given these strata the same general title. The question cannot be definitively answered in the absence of longitudinal evidence.

Strata 3 and 4 are termed "old middle classes" because they comprise occupations that require entrepreneurial skills rather than formal education; none of the men in these strata has a tertiary education, but their average occupational status and income compare favorably with those in the first two strata. Although they account for only one-fifth of the total sample, these two strata contain one-third of the self-employed and employers, two-thirds of the graziers, one-third of the managers, two-fifths of the shop proprietors, and two-thirds of the farmers. Thus these strata span a range of rural and urban occupations characterized by independent employment status, high income, but low formal education. The men in stratum 3 are distinguished from those in stratum 4 mainly by higher income. Stratum 4 is also slightly more heterogeneous than stratum 3, with 75% and 84%, respectively, of their status profiles falling within a close range of their modal profiles. Both have low stratum attribute consistency, mainly because of low education.

Strata 5 and 6 are difficult to interpret, and it can be argued that they *fall outside* the main stratification order. Both rank high on education (higher than the old middle classes), and both exhibit a variety of mixed status profiles. In fact, 22% of the observed status profiles (37 out of 170) but only 11% of the sample are found in these two strata. Unlike the other strata so far discussed, these strata do not seem to be differentiated by any age or life-cycle effects. On average, the men in stratum 5 are occupationally more successful than those in stratum 6, even though the latter have a higher average level of education. These two strata may consist of marginal men whose educational attainment has not been matched by typical occupational and financial rewards. But to some extent they are residual categories of men who do not fit the more dominant patterns of social differentiation.

Except for stratum 4, the six highest strata are smaller than the four lowest. They account for less than half (43%) of the total sample, but for all of the tertiary-educated men, most of those on incomes of $4,200 per annum or above (the top 30% of income earners), all the upper professionals and a majority of lower professionals, graziers, farmers, and

managers (88%, 97%, 69%, and 86%, respectively). The remaining four strata are relatively large, and each shades into the next without abrupt demarcations. Indeed, the main differences between the two middle-mass strata and between the two working-class strata seem to represent cohort effects in education and, to a lesser degree, in earnings.

The middle-mass strata (7 and 8) jointly account for one-third of the sample. About half their members are skilled manual workers (three-quarters of this occupational category are found in these strata), and about a fifth are lower-white-collar workers. Less well-off farmers and managers and higher-paid operatives make up most of the remainder, with farmers being somewhat more prominent in stratum 8 than in 7. The men in stratum 7 tend to be younger than those in stratum 8; 48% and 36%, respectively, are aged between 25 and 39 years The higher proportion of older men in stratum 8 (18%, or twice as many as in stratum 7, are 55 or older) accounts in part for the lower average income in this stratum. The earnings of manual workers tend to decline after about age 40. Men in stratum 7 have somewhat more education, reflecting the upward secular trend in school-leaving age during the first half of the century. The older men grew up at a time when the norm was to leave school at the end of primary education, whereas for younger men the norm (and the law) was to obtain a few additional years of secondary education. In terms of relative life chances among their age peers, there is little effective difference between these two strata, apart from cohort and stage in the life cycle. We would expect the earnings of the men in stratum 7 to decline as they grow older to about the relative level of the men in stratum 8. The somewhat higher education of men in stratum 7 conferred no competitive advantage on them in their careers relative to the lower education (normal for their time) experienced by the older men in stratum 8 These two strata could be aggregated into a single broad grouping without much information loss.

The last two strata consist overwhelmingly of the semiskilled and unskilled. Like the middle mass, they differ little from each other, although a finer classification not used in developing this typology shows that in stratum 9 there are more service workers and shop assistants, occupations that tend to be held by men starting out on their working lives. The age difference between men in strata 9 and 10 is marked, with 56% under 40 years of age in stratum 9 but only 31% in that age bracket in stratum 10. Thus the lower education and lower earnings of men in the last stratum can be explained in the same way as the differences between the two middle-mass strata, that is, as cohort and life-cycle effects. The status profiles of men in these strata are highly homogeneous: 98% and 95%, respectively, fall within a narrow band of differences from their modal patterns, which are semiskilled work, low income, and low educa-

tion. However, whereas all the men in stratum 9 had some secondary education, none of those in stratum 10 had progressed beyond primary school. This last stratum has very high stratum attribute consistency, as the last column of table 2 shows.

The foregoing descriptions open up the opportunity to assess how far these statistical strata exhibit consciousness of kind, as indexed by class self-identification and voting behavior, and closure in membership, as indexed by father-to-son mobility Given the exploratory nature of our analysis, we are more concerned to describe the ways in which our strata differ than to advance detailed working hypotheses. We should, however, point out that there are three possible kinds of effect on the dependent variables considered: general socioeconomic status, stratum homogeneity, and stratum attribute consistency.

Before presenting these analyses, we should make a few general observations about stratum formation in Australia in the mid-1960s. The correlations between different indicators of socioeconomic status are positive, so that on average a high position on one index implies a high position on the others. But they are far from perfectly correlated. Yet this lack of perfect symmetry should not distract us from the important fact that a small number of specific status profiles, some "consistent" and others "inconsistent" at the individual level, account for a very large proportion of the population. For example, if we ignore the two ambiguous strata (5 and 6), 83% of the rest of our sample have a profile which either is modal in a particular stratum, or differs from it by a small degree. Australian society is in this sense highly stratified, since only a small subset of the logically possible patterns of status profiles that occur account for most of our sample. The effects of structure are pervasive.[4] However, we should add that the existence of such a pervasive structure of social stratification says nothing in itself about the inheritance of unequal statuses from one generation to the next. On the contrary, crosscutting factors may mask the existence of strata defined strictly in socioeconomic terms: life-cycle effects such as the reduced earnings of manual workers in late career and secular changes such as the rise in the legal age of school leaving during the present century. Stage in life cycle and secular change cut across strictly socioeconomic differences and reduce the clarity with which social strata can be distinguished. Stage in life-cycle differences indicates that longitudinal as well as cross-sectional data are needed to assess the dynamics of stratum formation and recruitment.

[4] The only comparison we can make on this point is with Czechoslovakia, where Machonin (1969), using a slightly less restrictive definition of profile consistency, was able to account for 92% of his sample We guess that, were the two studies more nearly equivalent, the comparable Australian figure would be somewhere between 85% and 90%, which suggests that socioeconomic stratification in Czechoslovakia is no less pervasive than in Australia and might indeed be slightly greater.

STRATUM CONSCIOUSNESS

It is important to bear in mind that neither class identification nor voting behavior were explicit criteria in our definition of strata but are, instead, contingent properties, free to vary from one stratum to another. We expect differences in these two indicators of stratum consciousness to be related to three strata characteristics: general status, stratum homogeneity, and stratum attribute consistency. The first relationship is straightforward and amounts to an assertion that, at the individual level, how a person identifies his social class and how he votes is a function of his socioeconomic status. That individual relationship is bound to be reflected at the aggregate level, since the strata are constructed explicitly to capitalize on differences in socioeconomic profiles.

The other two relationships are of a different kind, since they involve a degree of reification of the strata which is problematic. Our general argument is that the more homogeneous a stratum and the higher its degree of stratum attribute consistency, the more likely are its members to perceive themselves as belonging to a stratum with different interests from other strata. Further, such a perception should lead to a heightened group awareness, over and beyond that attributable to their common socioeconomic status. Since our data are cross-sectional, we cannot offer a rigorous test of this inference, although we can assess whether the data fit with it. Major interpretative difficulties are that variations in general status between strata must be controlled before looking for other effects and that such aggregate effects as we hypothesize are likely to be modest (see Alexander and Eckland 1975, p. 403; Hannan 1971).

A casual inspection of table 3 shows that, within each pair of named

TABLE 3

General Status, Stratum Homogeneity, and Stratum Attribute Consistency in Relation to Self-identified Class and Voting Behavior in Australia (1960)

	Stratum	General Status	Stratum Homogeneity	Stratum Attribute Consistency	% Identifying Working or Lower	% Voting Australian Labor Party*
1	Upper middle class A	5 68	81 7	0 16	7 4	22 6
2	Upper middle class B	4 69	71 6	0 86	13 0	23 9
3	Old middle class A	4 63	84 5	1 10	22 0	14 3
4	Old middle class B	3 67	74 8	0 77	37 3	28 9
7	Middle mass A	3 14	77 3	0 16	46 7	48 0
8	Middle mass B	2 66	71 1	0 41	66 1	56 9
9	Working class A	2 19	98 2	0 59	70 4	64 8
10	Working class B	1 74	95 1	0 10	80 6	75 3

* In terms of the Australian party system, voting for the Australian Labor party is voting for the party of the Left. The basic split in Australian politics in the mid-1960s was between the Australian Labor party and the Conservative Liberal–Country party coalition. For a general account of the voting measure, see Broom and Jones 1970, pp. 992–93.

strata (all strata except the possibly marginal ones), higher general status is associated with higher stratum homogeneity, but there is no such simple relationship with stratum attribute consistency. Old middle class A has the highest degree of stratum attribute inconsistency but also relatively high stratum homogeneity. Taking all strata together, the relationship between general status and stratum homogeneity is curvilinear, being highest at the extremes of the status order, while stratum attribute consistency is virtually uncorrelated with general status; the two extreme strata have similarly high degrees of consistency, but there is no obvious pattern in the other strata. On the other hand, the percentage identifying with the working or lower class shows a uniform negative relationship to general status, to the extent that 97% of the variance in aggregate differences in class identification can be explained by general status.[5] Stratum homogeneity and stratum attribute consistency have insignificant additional effects, adding 1% to the explained variance. In no stratum, however, is there perfect agreement about class identification, but it is obvious that our typology is a highly informative classification of Australian workers: in the sample as a whole, the split in identification between upper or middle class and working or lower class is 51:49, but in our extreme strata the split is much more one-sided. Even so, significant proportions of blue-collar workers see themselves as middle class, and smaller numbers of white-collar workers see themselves as working class, possibly because their respective class schemes are either upper/middle/lower (i.e., exclude a working class) or upper/working (i.e., exclude a middle class). The existence of such differences points to factors in social consciousness that cut across objective status differences and to disagreement about the basis of social class differences and group affiliation, at least at a verbal level. Verbal differences may, of course, simply be alternative modes of describing the same underlying reality (see Davies 1967).

A largely parallel pattern of results is obtained when voting behavior is used as the index of stratum consciousness: general status accounts for almost all the aggregate variation in voting behavior (96%), with stratum homogeneity and stratum attribute consistency adding little extra explanation (1%). There are only two apparent differences from the results for class identification. First, there is one inversion in rank order, with the old middle class A being the stratum least likely to vote for the Australian

[5] In interpreting this apparently high figure, the reader should remember that no account is taken of within-stratum differences in class identification. Our analysis is here concerned with variations in group, not in individual, behavior (see Broom and Jones 1970, p. 994). Throughout this analysis, we use Pearsonian measures of correlation and regression. We have not reported the relative importance of the effects of each contextual variable, because the small number of cases and the high degree of collinearity among our explanatory variables render the interpretation of individual regression coefficients questionable.

Labor Party, a finding that reflects the antipathy of entrepreneurs and farmers to parties of the Left. Second, the typology is marginally less informative about voting patterns than about class identification: the split between Labor/non-Labor is somewhat less marked at the extremes (taking stratum 3 in this case as an extreme) than was the case with class identification. General socioeconomic status has a slightly closer relationship to class identification than to voting behavior. As table 3 shows, the Labor party cannot count on the votes of all those who term themselves working class, although it does gain votes from middle-class identifiers in the upper middle classes. In all other strata except middle mass A, there are fewer Labor party voters than working-class identifiers.

The analysis above suggests that the independent effects of stratum homogeneity and stratum attribute consistency are weak and of far less importance than general socioeconomic status in influencing class identification and voting behavior. Of course, our indexes of stratum consciousness are not ideal or exhaustive, and they relate to statistical aggregates rather than naturally occurring social groups. However, there are reasons to believe that such aggregate effects will be weak in a society where stratum membership varies over careers and between generations. In the absence of longitudinal data on stratum formation in Australia at earlier periods and because we have insufficient information on parents to construct origin strata for the parental generation, our present analysis and interpretation must remain rather tentative. Some might see in our findings on stratum consciousness the latency of strong group awareness; others, a modest evidence of manifestly unequal life chances. We tend to a low salience interpretation for several reasons. first, in a mass-consumption society social differentiation tends to be a regular gradient rather than an abrupt transition from a class of haves to a class of have-nots; second, income differences in Australia have tended to be smaller than in most other Western industrial countries, and third, high rates of father-to-son mobility prevent the maintenance of rigid boundaries between social aggregates for long periods.

STRATUM PERMEABILITY

Elsewhere we have given more detailed attention to father-to-son mobility (Broom and Jones 1969). For present purposes, we focus on gross mobility rates into each stratum, using the six broad occupational categories listed in the note to table 1. Because there are small numbers in any one of those six categories and because we can define social origins only in terms of father's job and not his stratum of origin, we are restricted to an inflow analysis designed to answer the question of how open or how closed each stratum is, in terms of its recruitment patterns.

TABLE 4

FATHER-TO-SON MOBILITY IN 10 SOCIAL STRATA, 1965

	% SONS			
STRATUM	Upwardly Mobile	Downwardly Mobile	Immobile	Mobile within Short Range*
1. Upper middle class A	70	4	26	28
2. Upper middle class B	62	20	18	30
3 Old middle class A	41	7	53	19
4. Old middle class B	51	9	40	17
7. Middle mass A	36	31	33	29
8 Middle mass B	44	26	30	29
9 Working class A	12	62	26	30
10 Working class B	16	55	29	25

* Son in occupational stratum immediately above or below father's

Table 4 shows a high rate of father-to-son mobility in the examined portions of the sample and in all strata except old middle class A (strata 5 and 6 are omitted, as in table 3). In other strata, two-thirds to three-quarters of the men in each stratum come from social origins that are higher or lower than would be expected on the basis of strict inheritance. Upward mobility is more common than downward mobility, because the occupational structure itself has been changing in the direction of higher-status jobs. However, as the final column of table 4 shows, most mobility is restricted to jobs with similar social standing: in the sample as a whole, one in four sons moved to an occupational stratum immediately above or below his father's, while another third remained in the father's stratum.

Inheritance is most marked in the old middle classes, which, as already noted, are characterized by entrepreneurship. Note also that the bottom two strata consist predominantly of *downwardly* mobile men, a finding which suggests that these lowest strata are not markedly self-perpetuating. However, our sample is restricted to the employed, and we have no data on the unemployed or the unemployable (about 2% of the work force in 1965). Among them there may be a cycle of self-perpetuating poverty, just as there may be a cycle of self-perpetuating wealth and power among the top few percent Nevertheless, for the bulk of the population, the prospect of mobility from one generation to the next is not an illusion, both because of low rates of occupational inheritance and because of the expansion of higher-status jobs. Father-to-son mobility undoubtedly restricts the growth of strong stratum consciousness and acts as a buffer to group definition. The effect of mobility in modifying the composition of social strata over time can be seen in table 5, which shows how each stratum varies in terms of social origins.

The four highest strata are characterized by larger than expected proportions of sons from professional backgrounds and the four lowest

TABLE 5

STRATUM CONSISTENCY IN TERMS OF SOCIAL ORIGINS* (1965) (Column Percentages)

FATHER'S OCCUPATION	STRATUM 1	2	3	4	5	6	7	8	9	10	TOTAL
Professional	24	16	22	10	12	9	4	4	4	6	9
Managerial	24	20	46	36	25	20	28	21	19	28	27
White collar	18	22	8	7	15	11	10	8	12	3	10
Skilled manual	13	25	7	13	19	20	24	26	19	15	18
Semiskilled	10	11	8	15	12	19	16	20	20	16	16
Unskilled	11	5	9	19	16	21	18	22	26	33	20
N	124	80	117	215	80	122	267	353	200	227	1,845
$\bar{X}$ score†	3.55	3.90	4.38	3.56	3.58	3.17	3.28	2.99	2.91	2.95	3.37
$\bar{X}$ deviation†	1.38	1.28	1.26	1.58	1.45	1.44	1.35	1.26	1.36	1.40	1.30

* Origin defined as father's occupation

† Scores and average deviations are calculated by giving a score of 6 to the highest category, 5 to the second highest, and so on to 1 for the lowest.

by larger than expected proportions of men from semiskilled or unskilled jobs. However, the average occupational status of each stratum is much closer to the sample average when social origins rather than present jobs are considered, and the diversity of social origins within each stratum is relatively great. Although men in higher strata tend to come from more favored social origins, the span of status inequality is much reduced when social origin rather than current position is considered. For example, in table 2 the largest difference between strata in average occupational status is just over four points on a six-point scale (strata 1 and 9), whereas, when father's job is considered, the maximum difference is only 1 5 points (strata 3 and 9). In short, even though the structural relationships among socioeconomic statuses such as education, occupation, and income generate relatively clearly defined social strata embracing a large proportion of the population at any single point in time, father-to-son mobility creates discontinuity between generations in the inheritance of structured inequality.

CONCLUSION

Australia is clearly a stratified society with relatively clear patterns of inequality in occupational position, skill and training, income, and other characteristics. The existence of status inconsistency at the individual level does not necessarily imply that internally homogeneous social strata can be identified, since many highly rewarded occupations (for example, managerial and entrepreneurial jobs and large-scale farming) have not traditionally required high educational qualifications, even though they may confer high monetary rewards. Some homogeneous strata are made up of individuals who would be termed status inconsistent. If their reference group were their stratum, as we have defined it, such persons would probably not perceive themselves as status inconsistent in a normative sense. The relative importance of the effects of stratum homogeneity and stratum attribute consistency on group awareness appears to be weak in relation to the effect of general socioeconomic status, a finding due partly to the consequences of social mobility and partly to the problematics of establishing how far the social strata we have defined are perceived as naturally occurring groups. Although high rates of father-to-son mobility limit the degree to which social strata can become self-perpetuating interest groups over time, we know of no data sources adequate for an unequivocal evaluation of the true effects of the aggregate variables we have defined on the emergence of social strata and the behavior of their members. To make such an evaluation would require longitudinal data not merely on individual statuses but also on group memberships and behaviors over careers and across generations.

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Trends in Residential Segregation: 1960–1970[1]

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The literature on racial residential segregation in American metropolitan areas reports contradictory findings on the decade of the sixties. Some researchers have concluded that average scores declined between 1960 and 1970, while others point to evidence of increases. This paper presents tract-based indexes for all 237 SMSAs (and their central cities) in 1970 and a comparable set of indexes for 1960. These are also cross-tabulated against region, population size, and minority proportion. Several conclusions are drawn: (1) overall, the data indicate a general decline in the average level of segregation between 1960 and 1970; (2) much of that decline is due to the relatively low scores among SMSAs added during the decade; (3) contradictory findings reported in the literature are likely to be due to sampling or other methodological inconsistencies; and (4) clear variations in levels of segregation persist with regard to region, population size, and minority proportion. The importance of these findings for future research is discussed.

With the passing of the 1960s, there was promise of improved black-white relations in the United States. Although the civil rights movement had begun to fade, it left behind a momentum for racial change. The Civil Rights Act of 1964, plus a wide range of legislation at all levels—federal, state, and local—gave expression to this possibility. Combined with this were improvements in housing conditions and economic welfare, as well as governmental commitments to urban renewal, antipoverty action, and assistance of minorities. Even the racial confrontations and riots in the latter half of the decade may have served to sensitize the public to the plight of blacks.

Without doubt there were improvements in the attitudes of whites toward blacks. Pettigrew's (1973) summary of local and nationwide surveys relating to attitudes on open housing concludes that "[w]hite American attitudes . . . have become increasingly more favorable" (p. 79).

[1] This is a revision of a paper presented at the annual meeting of the American Sociological Association in San Francisco, 1975 It reports on research supported by the National Science Foundation, grants G5,41243 and Soc-75-03438. Basic Data were provided through the Center for Demography and Ecology, University of Wisconsin The assistance of Karl Taeuber, Annmette Sørensen, and Barbara Aldrich is appreciated.

Even the widely publicized "white backlash" does not appear to have significantly altered the trend toward greater housing opportunities for blacks. Moreover, there have been similar changes in other realms, such as political participation, school integration, and employment (Campbell 1971; Greeley and Sheatsley 1971; Hermalin and Farley 1973).

Concurrent with changing attitudes, strides were made in reducing the socioeconomic differentials between nonwhites and whites.[2] Farley and Hermalin (1972) demonstrate that increases in income level during the decade were proportionately greater among nonwhites than among whites. Both groups increased their incomes by similar dollar amounts (whites by $2,688 and nonwhites by $2,530), but this progress represented only a 37.8% increase for whites in contrast to 69.1% for nonwhites. After a detailed analysis of occupational and educational trends between 1960 and 1970, Farley and Hermalin conclude: "Poverty was substantially reduced among blacks . . . and on a number of key socioeconomic characteristics, such as income, occupation, and education there was a significant upward shift of blacks. . . . In each case, differentials between whites and blacks narrowed" (pp. 365–66).

Whether these improvements signal noticeable changes in residential segregation patterns, however, is another question. More liberal white attitudes and better economic conditions have opened up housing opportunities for blacks. But it is equally clear that a pervasive "web of discrimination" continues to prevent blacks from free access to housing in most American communities (Foley 1973, pp. 95-107). Such discrimination hampers the possibility of fully integrated housing, despite improved white attitudes and black socioeconomic conditions.

The purpose of this paper is to examine changes in the residential segregation of blacks between 1960 and 1970. Our focus is on the nation's metropolitan areas, to ascertain not only the extent of change in segregation levels but the characteristics of those areas experiencing change. Fundamentally, the question is, Did residential segregation increase or decrease during the 1960s? We review recent conflicting evidence and seek to provide a more definitive answer to the question.

RECENT STUDIES IN RESIDENTIAL SEGREGATION

The publication of Taeuber and Taeuber's *Negroes in Cities* in 1965 was a major contribution to research on the residential segregation of blacks in the United States. For the period up to and including the 1960 census, they summarized the literature and provided a broad-scale analysis of

[2] Although the data substantiating this statement refer to the more inclusive category of nonwhites and not just blacks, the latter constitute the vast majority of nonwhites. Hence, the same conclusion would be likely even with the more detailed information.

segregation patterns by race. They concluded that a high degree of segregation existed at that time in virtually all American cities. They also noted that the increases in residential segregation in all regions during the forties were, with the exception of the South, followed by declines during the fifties (Taeuber and Taeuber 1965).

Since the publication of *Negroes in Cities,* only a handful of papers have attempted to follow the analysis of patterns of racial residential segregation through the 1960s and into the 1970s. Before data were available from the 1970 census, Clemence (1967) and Farley and Taeuber (1968) speculated about trends in segregation on the basis of small samples of cities that had received special mid-decade enumerations of their populations. They concluded separately that no abatement in overall levels of segregation appeared to have occurred since Taeuber and Taeuber's assessment for 1960. There were even indications that it was *increasing* (see Clemence 1967, p. 568; Farley and Taeuber 1968, pp. 153–55).

Recently, however, Sørensen, Taeuber, and Hollingsworth (1975) have released figures pertaining to 109 central cities. They found an overall *decrease* in the degree of segregation during the sixties. This, combined with previously reported declines during the fifties, brought the 1970 average below that of 1940. Even in the South, where resistance to integration had been strongest, the sixties saw a moderate decline in segregation (see also Roof, Van Valey, and Spain 1976).

These findings are clearly at variance with the mid-1960s studies, and for reasons we can speculate about. Substantial changes may have occurred for the nation as a whole since the mid-1960s, reversing the apparent trend toward increasing residential segregation observed by Clemence (1967) and Farley and Taeuber (1968). This, however, is unlikely. More reasonable explanations are methodological in character. First, the small samples of the Clemence and the Farley and Taeuber studies ($N = 10$ and $N = 13$, respectively) may well have been biased, with disproportionate regional or size representation and possibly variances that differed markedly from those of the Sørensen et al. (1975) sample. Second, differences in unit definition (e.g., changes in city boundaries and annexation of new areas to the central cities) may have produced different segregation scores as an artifact of measurement. Third and finally, differences in the use of blocks versus tracts have previously been shown to be responsible for inconsistent findings (see Roof and Van Valey 1972). Such a possibility must also be considered in this case.

Although the paucity of mid-decade data on residential segregation precludes a direct and thorough assessment of changes between 1965 and 1970, it is possible to construct 1960 and 1970 national samples and to examine more systematically the trends over the full 10-year interval. This procedure does not, of course, solve all the problems of intercity

comparability, especially annexations and boundary changes. It does provide considerable control over the variations in sampling and measurement, however, thus eliminating major sources of difficulty. We have proceeded in this way, calculating separate segregation scores for central cities and SMSAs for each of the 237 fully tracted metropolitan areas in 1970 as well as for those similarly available for 1960.

MEASURING SEGREGATION TRENDS

An index of racial residential segregation may be calculated for a given geographical area on the basis of the numbers of white and minority individuals or households in that area. The measure employed here is the conventional index of dissimilarity, which is computed according to the following formula:

$$D = 1/2 \left(\sum_{i=1}^{N} | x_i - y_i | \right) \times 100,$$

where each i is a census tract and N tracts constitute the particular unit (central city or SMSA) for which the index (D) is computed; x_i and y_i are the percentages of blacks and whites, respectively, in the ith tract. Because it is based on the absolute percentage differences, calculated across all tracts in a given unit, the index gives an easily interpretable estimate of the overall extent of racial residential segregation, with scores ranging in value from 0 (indicating proportional numbers of blacks and whites in *all* tracts) to 100 (indicating that no tract in the unit has both blacks and whites living in it).

Despite its wide usage in segregation research, the index of dissimilarity is subject to certain limitations. Perhaps the most important is the fact that it does not take into consideration the spatial location of residences (black or white) within the unit Consequently, while it does provide estimates of the unevenness of the two population distributions and of the proportions of the two populations that would be required to move in order to achieve an even areal distribution, it gives no information on racial residential clusters or "ghettos" and cannot tell us either how far or in what direction the redistribution should take effect (see Taeuber and Taeuber 1965, appendix A). This could be a serious limitation. However, since we are interested in summary measures of residential segregation rather than the process of ghettoization itself, the index appears to be appropriate for our purposes.

Another consideration is the fact that, unlike the indexes used by Taeuber and Taeuber (1965) and by Sørensen et al. (1975), those reported below are calculated on the basis of census tracts instead of blocks. This decision was made in spite of the fact that past research has shown

the block-based indexes to be slightly more indicative of actual population distributions than those based on tracts (Taeuber and Taeuber 1965; Roof and Van Valey 1972).[3] Considerations of sampling and index comparability determined our choice, however. Tract-based indexes are available for *all* of the metropolitan areas in 1970 as well as a large number of those that were identified in 1960. Moreover, for both 1960 and 1970, they have the advantage of allowing separate comparisons of segregation levels among central cities and entire metropolitan areas (plus suburban rings—not reported here).

Indexes for 1970 were based on abstracts of the Second Count Summary tapes. Those for 1960 were calculated from the published volumes of the 1960 census. In preparing the two sets of segregation scores, we sought to correct the major coding and computational errors. Indexes for 1970 were considered to be correct only if the black, white, and total population figures on which they were based matched exactly the totals reported in the published volumes. All discrepancies were calculated and expressed as percentages of the reported totals. For the complete set of 237 SMSAs there were only 11 such discrepancies, with an average "error" of 0.09%. Central-city figures were slightly more troublesome, containing 47 discrepancies and an average error of 1.86%. Coding errors for the 1960 data, after double-checking, were also presumed to be negligible.

SEGREGATION INDEXES FOR 1970 AND 1960

Table 1 presents the tract-based indexes of residential segregation for SMSAs and central cities for each of the 237 metropolitan areas in the coterminous United States which were fully tracted in 1970, and for the 137 comparable places in 1960.[4] The mean index scores for SMSAs are

[3] For 1960, overall block and tract segregation scores for 144 central cities were correlated at a .58 level; the regression coefficient of block scores on tract scores was .40 (Van Valey and Roof 1976). Moreover, this relationship fluctuated considerably with variations in city size and proportion nonwhite. A similar comparison, based on scores for 81 central cities in 1970, shows some improvement in the overall relationship, with correlation and regression coefficients of .77 and .58, respectively. Given that the coefficients are only moderately large, it is clear from this information that the two indicators do not measure residential segregation equally well. In addition, given the absence of evidence to the contrary, some effects of city size and proportion nonwhite, as noted above, must be assumed for the 1970 data. Thus, any comparison of block and tract scores, especially those using samples of cities, must be interpreted with caution. Furthermore, it is clear that there is a need for further research concerning the precise manner in which these (and other) measures of residential segregation are related.

[4] Seven cities were deleted from the total of 144 available for 1960 because they either were second central cities of multi-central-citied SMSAs (e.g., Saint Paul) or were later incorporated into existing SMSAs (e.g., High Point, N.C.). Their SMSA indexes are listed in table 1. The central-city indexes are as follows: East Chicago, 74.2; High Point, 70.3; North Little Rock, 67.7; Riverside, 75.8; Saint Paul, 86.3; Saint Petersburg, 93.4; Winston-Salem, 83.3.

TABLE 1

INDEXES OF RESIDENTIAL SEGREGATION FOR SMSAs AND CENTRAL CITIES 1970 AND 1960

	SMSAs		CCs			SMSAs		CCs	
	1970	1960*	1970	1960*		1970	1960*	1970	1960*
Abilene	58.4	83.5	62.1	83.5	Boise City	58.4	...	57.9	...
Akron	77.2	78.6	73.0	75.5	Boston	79.3	80.8	81.2	83.9
Albany, Ga.	79.1	...	82.9	...	Bridgeport	73.1	66.3	64.5	53.6
Albany, N.Y.	67.1	68.6	65.7	66.8	Bristol	37.9	...	35.2	...
Albuquerque	51.1	70.5	51.4	75.3	Brockton	37.6	...	38.0	...
Allentown	65.6	...	60.1	...	Brownsville	51.5	...	40.8	...
Altoona	67.0	...	66.4	...	Bryan	68.6	...	81.7	...
Amarillo	86.2	...	83.5	...	Buffalo	85.7	86.8	83.4	84.5
Anaheim	72.3	...	45.7	...	Canton	77.3	74.6	76.4	71.9
Anderson	82.2		72.2	...	Cedar Rapids	65.6	...	62.8	...
Ann Arbor	51.9	72.6	41.1	62.8	Champaign	52.1	...	53.7	...
Appleton	69.5	...	59.1	...	Charleston, S.C.	62.8	62.8	80.3	72.2
Asheville	69.1		71.1	...	Charleston, W. Va.	65.3	...	54.3	...
Atlanta	81.7	77.1	83.4	83.1	Charlotte	72.3	75.6	88.7	87.1
Atlantic City	76.5	84.0	86.8	84.0	Chattanooga	72.3	77.3	71.9	76.8
Augusta	58.6	72.2	74.6	80.0	Chicago	91.2	91.2	91.0	91.8
Austin	73.5	63.3	76.1	66.8	Cincinnati	81.8	83.2	74.3	81.2
Bakersfield	75.6	72.7	85.6	83.4	Cleveland	90.2	89.6	86.7	85.6
Baltimore	81.0	82.4	84.3	83.0	Colorado Springs	55.0	...	59.8	...
Baton Rouge	74.3	68.7	78.6	75.5	Columbia, Md.	59.5	...	62.2	...
Bay City	76.4	...	67.8	...	Columbia, S.C.	63.5	63.6	66.4	76.4
Beaumont	78.6	77.5	72.5	70.3	Columbus, Ga.	61.6	78.4	70.2	81.1
Billings	52.1	...	48.1	...	Columbus, Ohio	80.9	76.1	77.5	70.0
Biloxi	59.5	...	60.0	...	Corpus Christi	68.9	66.2	72.9	71.2
Binghamton	46.7	...	46.2	...	Dallas	86.9	81.2	91.7	88.8
Birmingham	67.6	64.1	70.9	69.0	Davenport	73.7	...	74.5	...
Bloomington	53.5	...	46.3	...	Dayton	87.3	90.8	88.2	88.3

TABLE 1 (*Continued*)

	SMSAs 1970	SMSAs 1960*	CCs 1970	CCs 1960*
Decatur	71.4	66.9	64.3	55.5
Denver	84.7	84.6	84.6	83.4
Des Moines	74.5	77.0	71.7	77.4
Detroit	88.9	87.1	78.2	80.4
Dubuque	57.8	...	57.7	...
Duluth	65.6	...	57.8	...
Durham	63.2	65.5	72.5	80.0
El Paso	51.0	64.5	47.7	65.7
Erie	72.0	72.3	64.0	66.9
Eugene	52.3	...	26.8	...
Evansville	75.2	68.4	81.5	76.8
Fall River	39.8	...	35.8	...
Fargo	49.3	...	32.7	...
Fayetteville	44.5	...	78.8	...
Fitchburg	41.1	...	42.7	...
Flint	86.4	83.0	77.7	81.9
Ft. Lauderdale	94.9	...	97.6	...
Ft Smith	66.3	72.2	74.8	72.2
Ft Wayne	82.3	85.4	79.3	80.2
Ft. Worth	86.8	84.8	82.8	84.7
Fresno	73.2	77.4	79.6	87.8
Gadsden	68.4	62.1	67.2	59.0
Gainesville	63.3	...	81.6	...
Galveston	72.1	64.5	68.6	70.3
Gary	88.2	89.1	78.4	85.8
Grand Rapids	84.7	83.9	80.1	81.4
Great Falls	67.0	...	64.8	...
Green Bay	69.9	...	47.4	...

	SMSAs 1970	SMSAs 1960*	CCs 1970	CCs 1960*
Greensboro†	74.6	66.9	83.6	84.0
Greenville	42.7	38.1	59.1	44.5
Hamilton	82.6	79.3	85.7	87.3
Harrisburg	78.4	80.9	64.1	74.0
Hartford	83.1	80.2	74.7	77.3
Houston	78.4	80.5	82.2	80.4
Huntington	76.4	...	73.5	...
Huntsville	57.3	...	62.7	...
Indianapolis	83.8	78.7	80.3	76.0
Jackson, Miss	80.9	80.2	67.1	65.0
Jackson, Mo	70.1	...	87.9	...
Jacksonville‡	82.4	77.7	82.4	77.7
Jersey City	73.0	72.5	66.0	62.0
Johnstown	78.8	...	65.6	...
Kalamazoo	71.3	76.2	72.9	70.5
Kansas City	87.4	83.3	84.2	80.6
Kenosha	69.9	...	64.1	...
Knoxville	78.3	77.8	79.9	77.8
Lafayette, La	57.6	...	71.4	...
Lafayette, Ind.	52.4	...	58.8	...
Lake Charles	68.8	...	80.1	...
Lancaster	75.2	...	74.5	...
Lansing	64.8	83.3	58.8	80.6
Laredo	59.9	...	60.1	...
Las Vegas	84.8	97.6	93.3	97.6
Lawrence	50.0	...	46.0	...
Lawton	52.8	...	64.9	...
Lewiston	48.6	...	37.7	...

TABLE 1 (*Continued*)

	SMSAs 1970	SMSAs 1960*	CCs 1970	CCs 1960*		SMSAs 1970	SMSAs 1960*	CCs 1970	CCs 1960*
Lexington	78.3	69.8	79.6	62.3	Newark	78.8	72.8	72.1	63.2
Lima	79.5	69.6	70.9	62.2	New Bedford	64.2	...	61.1	...
Lincoln	60.9	...	59.9		New Britain	52.9	...	43.8	...
Little Rock	70.8	65.0	76.7	65.2	New Haven	67.0	65.4	73.0	53.5
Lorain	70.7	67.0	58.3	56.0	New London	53.2	...	46.0	...
Los Angeles	88.5	89.2	88.6	85.4	New Orleans	74.2	65.0	70.9	67.7
Louisville	82.8	80.4	84.4	79.5	Newport News	70.5	72.8	80.3	72.8
Lowell	50.1	...	58.3	...	New York	73.8	74.4	71.6	75.2
Lubbock	78.0	93.0	89.4	93.0	Norfolk	77.3	77.0	86.4	87.5
Lynchburg	47.6		65.2		Norwalk	64.0	58.8	58.0	58.8
Macon	57.0	55.8	60.1	56.0	Odessa	86.2	85.7	84.6	84.6
Madison	52.0	...	46.2		Ogden	73.6	...	70.8	...
Manchester	34.0	...	35.8	...	Oklahoma City	90.6	91.4	92.6	93.3
Mansfield	80.1	...	64.5	...	Omaha	82.7	88.9	84.1	88.8
McAllen	45.8		55.4	...	Orlando	83.8	84.7	92.7	95.7
Memphis	78.9	72.7	84.4	79.7	Oxnard	66.3	...	43.3	...
Meriden‡	40.8		40.8	...	Paterson	79.9	77.1	55.6	57.3
Miami	85.7	89.5	84.3	90.7	Pensacola	65.1	...	73.1	...
Midland	85.7		84.8	...	Peoria	78.6	85.4	70.2	80.2
Milwaukee	89.5	90.4	87.0	88.4	Philadelphia	78.0	77.1	76.8	79.0
Minneapolis	79.9	83.3	74.6	75.8	Phoenix	75.4	81.1	77.7	85.4
Mobile	68.5	73.2	82.9	73.2	Pine Bluff	64.2	...	70.5	...
Modesto	73.5	...	76.5	...	Pittsburgh	74.5	74.4	79.2	81.1
Monroe	76.9	86.2	92.4	86.0	Pittsfield	61.1	...	64.1	...
Montgomery	60.5	73.1	75.8	80.3	Portland, Maine	56.8	...	42.1	...
Muncie	84.5	87.1	85.8	86.3	Portland, Oreg	80.2	81.3	77.4	79.6
Muskegon	80.5	...	65.8	...	Providence	72.7	72.8	66.2	64.3
Nashville	77.3	76.4	80.1	71.6	Provo	48.2	...	45.9	...

TABLE 1 (*Continued*)

	SMSAs		CCs	
	1970	1960*	1970	1960*
Pueblo	50.5	..	48.6	..
Racine	76.2	...	70.4	...
Raleigh	59.7	56.7	81.0	84.6
Reading	71.8	72.4	56.5	61.0
Reno	66.2	..	68.0	...
Richmond	76.6	74.9	83.2	79.5
Roanoke	76.8	.	81.9	...
Rochester, Minn.	64.8	...	18.5	...
Rochester, N Y.	72.6	80.0	71.0	75.2
Rockford	80.2	69.5	76.2	67.6
Sacramento	66.1	72.1	61.0	60.1
Saginaw	83.7	81.6	78.4	78.8
St Joseph	56.0	...	51.1	...
St Louis	86.5	85.9	83.8	85.4
Salem	53.3	.	54.1	..
Salinas	63.0	..	39.3	..
Salt Lake City	70.1	71.6	57.9	67.9
San Angelo	67.0	..	66.0	..
San Antonio	74.0	76.8	80.6	80.8
San Bernardino	66.8	71.8	73.0	71.4
San Diego	76.2	79.5	76.1	80.2
San Francisco	77.3	79.4	67.8	71.1
San José	51.1	65.6	50.7	57.5
Santa Barbara	48.3	.	50.9	. .
Santa Rosa	50.3	..	54.8	. .
Savannah	77.9	70.6	80.5	78.8
Scranton	75.0	.	71.0	. .
Seattle	78.1	83.3	76.7	82.2

	SMSAs		CCs	
	1970	1960*	1970	1960*
Sherman	55.5	. .	57.9	...
Shreveport	77.9	73.2	88.7	82.7
Sioux City	69.8	...	69.7	..
Sioux Falls	50.7	...	48.3	...
South Bend	77.2	78.7	65.9	71.0
Spokane	60.9	65.6	59.5	66.7
Springfield, Ill.	80.0	.	75.0	...
Springfield, Mass	77.4	...	70.5	...
Springfield, Mo	71.4	65.1	68.9	62.7
Springfield, Ohio	68.6	61.1	58.1	48.0
Stamford	67.4	60.7	62.3	56.7
Steubenville	67.9	..	67.1	...
Stockton	75.5	76.9	70.6	64.7
Syracuse	77.6	78.5	66.2	77.8
Tacoma	57.4	71.2	59.6	69.4
Tallahassee	65.5	...	75.6	...
Tampa	84.5	83.6	82.8	78.0
Terre Haute	66.0	...	49.1	...
Texarkana	39.0	...	44.3	...
Toledo	85.0	78.7	82.3	81.0
Topeka	58.6	56.3	56.7	58.5
Trenton	68.1	59.1	63.0	51.8
Tucson	63.6	73.0	64.0	78.3
Tulsa	85.5	88.6	89.8	93.0
Tuscaloosa	53.2	...	64.6	...
Tyler	58.9	..	80.7	...
Utica	64.8	...	55.0	...
Vallejo	63.0	..	52.4	...

TABLE 1 (*Continued*)

	SMSAs		CCs			SMSAs		CCs	
	1970	1960*	1970	1960*		1970	1960*	1970	1960*
Vineland	54 4	...	33 4		Wichita Falls	78 6	84 0	78 8	84.0
Waco	68 2	62 3	78 0	66 7	Wilkes-Barre	75 7	...	51 9	...
Washington, D C.	81 1	77 7	72 3	66 4	Wilmington, Del	68 0	67 4	60.4	67.9
Waterbury	72 0	69 9	65 9	61 8	Wilmington, N.C.	59 6	..	78 2	...
Waterloo	85 7	82 2	79 7	76 0	Worcester	65 1	...	57 9	...
W Palm Beach	85 0	..	90 7	...	York	75 8	65 1	50 1	56.1
Wheeling	55 7	. .	69 0	.	Youngstown	79 5	75 3	68 1	65 4
Wichita	87 0	88 5	87 8	86 9					

* Indexes for 1960 are based on whites and nonwhites, those for 1970 are based on whites and blacks
† Winston-Salem was added to the Greensboro–High Point SMSA in 1970 Its SMSA score for 1960 is 81 7
‡ Central city comprises entire SMSA

75.4 for 1960 and 69.5 for 1970, a decline of 5.9 points (8% of the 1960 value). For central cities, the mean scores for 1960 and 1970 are 75.1 and 68.3, respectively, a decline of 6.8 points (9%). The scores for SMSAs range from 38.1 (Greenville, S.C.) to 97.6 (Las Vegas, Nev.) in 1960, and from 34.0 (Manchester, N.H.) to 94.9 (Fort Lauderdale, Fla.) in 1970. This represents a decrease of 2.7 points in the maximum and 4.1 points in the minimum. For central cities, the maximum score remained 97.6 (Las Vegas, Nev, 1960; Fort Lauderdale, Fla., 1970), but the minimum score dropped precipitously from 44.5 (Greenville, S.C.) to 18.5 (Rochester, Minn.), a decrease of 26 points.

If, however, we look at the SMSAs with the highest levels of segregation (scores of 80.0 or more), there is a striking consistency between 1960 ($N = 48$) and 1970 ($N = 50$). Although their relative positions within the ranks of the 10 most segregated are somewhat shuffled about (Oklahoma City remained constant; five others—Chicago, Cleveland, Milwaukee, Los Angeles, and Gary—experienced slight increases in rank; Dayton declined), seven of the "top 10" in 1960 are also in the "top 10" in 1970 (the same holds true for 13 of the top 20). Two others, Detroit and Kansas City, which were at positions 14 and 30 in 1960, had moved to sixth and ninth by 1970 Las Vegas and Miami, which had been first and eighth, respectively, in 1960, dropped to twenty-fifth and nineteenth by 1970. Fort Lauderdale, the SMSA with the highest level of segregation in 1970, was the only one of the top 10 not in the 1960 set of SMSAs. (Only eight of the top 50 SMSAs were added between 1960 and 1970.)

Turning to the SMSAs with the lowest levels of segregation, we find that direct comparison is more problematic: 58 SMSAs in 1970, but only six in 1960, had scores below 60 0 points. What is more, of the 58 in 1970, only 11 were even available for 1960, regardless of scores; the remainder were SMSAs added during the decade. Greenville, for example, the lowest in 1960, was eighth from the lowest in 1970, and the only SMSA in the 1970 "bottom 20" available in 1960. In fact, the next lowest score in 1960 (55.8—Macon, Ga.) is higher than the 41 lowest scores in 1970. Clearly a comparison of the SMSAs with the lowest levels of segregation reveals a striking inconsistency.[5]

Given this interesting mixture of consistent and inconsistent findings, in an attempt to specify more accurately the differences between the 1960 and 1970 segregation patterns, we explored trends by categories of region, population size, and minority proportion. Table 2 presents mean indexes

[5] The pattern observed for central cities is, as before, quite similar to that of the SMSAs Although the particular cities vary somewhat, the aggregate result is almost the same. In this case, among the highest-ranking cities, seven of 10 and 14 of 20 matched between 1960 and 1970 For the lowest, only nine of the bottom 50 in 1970 were available for 1960.

TABLE 2

MEAN INDEXES OF RESIDENTIAL SEGREGATION FOR SMSAs AND CENTRAL CITIES BY CATEGORIES OF SELECTED VARIABLES: 1960 AND 1970

	SMSAs			
	1970	1960	Average Difference	1970 Additions
Total	69 5 (237)	75.4 (144)	—5.9	61 8 (100)
Region:				
South	69 8 (86)	73 5 (58)	—3.7	64 1 (32)
West	65.8 (36)	76 3 (20)	—10.5	59 6 (17)
North Central	74 1 (66)	79 5 (42)	—5 4	65 5 (26)
Northeast	65 2 (49)	72 9 (24)	—7.7	56 6 (25)
Size (thousands):*				
50–99	60 2 (21)	69 7 (4)	—9 5	58 4 (19)
100–249	63 9 (91)	71 3 (48)	—7 4	60 3 (60)
250–499	70 4 (60)	75 4 (36)	—5 0	67 3 (16)
500–999	78 0 (32)	77 8 (31)	+0 2	76 2 (4)
1,000–1,999	79 4 (21)	82 3 (15)	—2.9	72 3 (1)
2,000 or more	82.5 (12)	81 7 (10)	+0 8	.. (0)
Minority proportion †				
0 0–0.9	57.5 (30)	(0)	...	57 0 (29)
1 0–4 9	66 6 (66)	74 9 (32)	—8 3	62 9 (35)
5 0–9 9	75 1 (52)	77 4 (47)	—2 3	67 6 (11)
10 0–19.9	76 9 (49)	77 9 (33)	—1 0	69 5 (10)
20 0–29 9	65 8 (28)	72 2 (20)	—6 4	55 8 (10)
30 0 or more	69 1 (12)	69 2 (12)	—0 1	65 7 (5)

	CENTRAL CITIES			
	1970	1960	Average Difference	1970 Additions
Total	68 3 (237)	75 1 (144)	—6 8	59.9 (100)
Region:				
South	75 7 (86)	77 2 (58)	—1 5	71 6 (32)
West	62 9 (36)	76 2 (20)	—13.3	53 4 (17)
North Central	68 5 (66)	75 9 (42)	—7 4	58.2 (26)
Northeast	59 1 (49)	57 7 (24)	—8 6	51 2 (25)
Size (thousands):*				
50–99	62 2 (116)	71 3 (45)	—9 1	58 9 (85)
100–199	70 4 (63)	73 3 (42)	—2 9	65 6 (15)
200–299	74 1 (14)	77 2 (18)	—3.1	... (0)
300–399	79 6 (12)	82 5 (10)	—2.9	... (0)
400–499	74 4 (6)	77 5 (9)	—3 1	... (0)
500 or more	80 7 (26)	80 9 (20)	—0.2	... (0)
Minority proportion †				
0 0–0 9	48 4 (25)	81 1 (1)	—32 7	48 4 (25)
1 0–4 9	57 8 (39)	70 2 (13)	—12 4	56 7 (30)
5 0–9 9	69 2 (41)	74 7 (45)	—5.5	60 5 (13)
10 0–19 9	72 6 (57)	75 2 (33)	—2.6	69 4 (18)
20 0–29.9	75 5 (36)	76 1 (27)	—0.6	70.4 (7)
30 0 or more	77 7 (39)	76 9 (25)	—0 8	78.8 (7)

NOTE See n 4 above for explanation of discrepancy in total figures
* Two cities, Atlantic City and Jackson, Miss , dropped below 50,000 total population in 1970 but were nevertheless included in census publications. They will be retained in trend analyses and placed in the 50,000–100,000 category
† Percentage nonwhite in 1960 but percentage black in 1970

for the full 1960 and 1970 sets of SMSAs and central cities, plus those added during the decade ($N = 100$).

Mean segregation scores in all regions declined during the decade, with the greatest decrease occurring in the West (11 points for SMSAs, 13 for central cities), followed by the Northeast, North Central, and the South (with declines of almost four points for SMSAs and two for central cities). Decreases were most noticeable among the smaller SMSAs and cities; among the very largest, in fact, there was virtually no change in overall segregation levels. Likewise, 1960–70 changes were most pronounced in SMSAs and central cities with small minority proportions. Segregation levels declined sharply in these places but were generally unabated in places where minority proportions were extremely high.

Table 2 also shows the summary scores for SMSAs and central cities added during the decade. It is obvious that, in every subcategory of region, size, and minority proportion but one (central-city minority proportion in excess of 30.0%), average scores for these newer places are lower than for those in 1960, in some cases substantially so. This raises an interesting question: To what extent are the substantially reduced segregation levels in 1970, reported by Sørensen et al. (1975) and presented in tables 1 and 2 a reflection of the addition of these new metropolitan areas?

To examine this issue, we looked first at the sample distributions. The distribution of places by region is remarkably similar for the two samples, with the proportion of cities changing no more than 1% in the West and North Central regions and at most no more than 4% (in the South and Northeast). Changes in population size and minority proportion are more significant. Although the mean size of central cities increased slightly (from about 232,000 to about 249,000), there was a substantial decrease in mean SMSA population size (from approximately 725,000 to 584,000). Moreover, as expected, the average size of the SMSAs added between 1960 and 1970 was relatively small—approximately 190,000. Similarly, because of the sharp increase in the number of places with a minority proportion of less than 1%, the proportion of SMSAs where blacks constituted more than 10% of the population dropped by 7.5%; for central cities the decline was 3.3%.

The inclusion of "new" SMSAs and central cities, therefore, is likely to affect central tendency scores systematically. The underlying relationships between residential segregation and its structural correlates, especially population size and minority proportion, differ for the two time periods; hence, direct comparisons may be misleading without proper adjustment procedures.[6]

[6] With regard to the remaining presentation of findings, there is a continuing high degree of similarity between the SMSA and the central-city units In the interest of conserving space, therefore, the balance of the paper will focus on the SMSA unit.

ANOTHER LOOK AT THE DATA A CONSISTENT SAMPLE

By using only the 137 available places which were fully tracted SMSAs both in 1960 and 1970, we obtain a different and more consistent perspective on segregation changes during the 1960s. For example, table 2 shows the overall SMSA scores to have declined from 75.4 in 1960 to 69.5 in 1970, a net change of —5.9 points, or 8.5%. This trend disappears when we look at table 3, which contains the mean segregation scores by region

TABLE 3

REGION AND TRENDS IN SEGREGATION FOR SMSAs: 1960 AND 1970

Region	N	1970 Index	1960 Index	Average Difference	% Change
South Atlantic	22	71.2	71.0	+0.2	0.3
East South Central	10	73.3	72.7	+0.6	0.8
West South Central	22	75.3	76.3	−1.0	−1.3
South	54	73.3	73.5	−0.2	−0.3
Mountain	6	71.6	79.7	−8.1	−10.2
Pacific	13	71.3	75.8	−4.5	−5.9
West	19	71.4	77.0	−5.6	−7.3
West North Central	9	79.3	78.9	+0.4	0.5
East North Central	31	79.8	79.4	+0.4	0.5
North Central	40	79.7	79.3	+0.4	0.5
Middle Atlantic	16	75.2	74.8	+0.4	0.5
New England	8	72.3	69.3	+3.0	4.3
Northeast	24	74.2	73.0	+1.2	1.6
Total	137	75.1	75.6	−0.5	−0.7

for the 137 SMSAs in our consistent sample. Here, the change is from 75.6 in 1960 to 75.1 in 1970, a very small net change of —0.5 points, or less than 1%. In fact, fully 70 of the metropolitan areas in the consistent sample (51.1%) experienced net increases in residential segregation between 1960 and 1970, in comparison with 66 that decreased and one that remained the same.[7] Had there not been many more decreases of large magnitude than comparable increases (11 in contrast to three), we would

Readers with particular concern for the central-city units can acquire comparable central-city tables from any of the authors

[7] This can be compared with 72 (66.1%) cities that declined in the Sørensen et al. (1975) sample of 109 cities. A more appropriate comparison, however, is provided by our central-city indexes, which show 72 cities (52.6%) decreasing, 64 increasing, and one stable All of these figures are based on a white-black index for 1970 and a white-nonwhite index for 1960.

even have seen an overall increase in segregation during the decade. As it is, by omitting the new SMSAs (see table 3), we observe a regional pattern different from that displayed in table 2. The Northeast metropolitan areas experienced moderate increases in segregation, the North Central experienced slight increases, and in the South the decrease was quite small. In fact, only three of the nine subregions declined in SMSA segregation: the Mountain, Pacific, and West South Central. Moreover, the large decreases in the two former regions were primarily responsible for the 0.7% decrease in the national average. Five regions declined in central-city segregation (not shown), but again, only the far western regions saw marked decreases. Clearly, there is no support in the present data for claims of widespread downward trends in residential segregation.

Turning to table 4, which examines the relationship between segregation and population size, we see an overall tendency for mean scores to rise as

TABLE 4

POPULATION SIZE AND TRENDS IN SMSA SEGREGATION 1960 AND 1970

Size (Thousands)	1970	1960	Difference	% Change
		A Consistent Sample		
50–99	77.3 (2)	69.7 (4)	+7.6	10.9
100–249	71.0 (31)	71.4 (45)	−0.4	0.6
250–499	71.6 (44)	75.4 (36)	−3.8	5.0
500–999	78.3 (28)	78.0 (28)	+0.3	0.4
1,000+	80.8 (32)	82.0 (24)	−1.2	1.5
Total	75.1 (137)	75.6 (137)	−0.5	0.7
		B Stable Growth Sample		
50–99	77.3 (2)	73.9 (2)	+3.4	4.9
100–249	71.4 (29)	72.2 (29)	−0.8	1.1
250–499	71.7 (29)	74.5 (29)	−2.8	3.7
500–999	77.9 (20)	78.1 (20)	−0.2	0.2
1,000+	82.3 (24)	82.0 (24)	+0.3	0.4
Total	75.4 (104)	76.3 (104)	−0.9	1.2

population size increases (scores range from 69.7 to 82.0 in 1960 and from 71.0 to 80.8 in 1970; see pt. A). This tendency holds for both 1960 and 1970 and is congruent with our earlier findings. The single exception is the category of SMSAs with fewer than 100,000 people. Looking at changes during the decade, however, we find a pattern differing substantially from that observed earlier. Table 2 indicates that the major changes occurred in the smallest places. The consistent sample, however, shows that the greatest decreases in segregation took place among middle-sized SMSAs. Among

the smallest SMSAs and those with 500,000 to 1 million inhabitants, there are, in fact, some noticeable increases.

Unfortunately, the metropolitan growth that occurred during the decade complicates such analyses. Between 1960 and 1970, 33 SMSAs changed size categories, 20 of them increasing in total population by at least 30%. None declined in size.[8] The change in segregation in these SMSAs was mixed, however, with 21 experiencing increased segregation (an average of 5.0 points) and 12 declining (an average of 5 9 points) Weighting these changes by their average frequencies, we find an aggregate increase in residential segregation of 1 0 point for the growing SMSAs. This trend, of course, is consistent with the overall direct relationship between size and segregation noted above.

Part B of table 4, in contrast, contains data for only those places that were relatively stable in size between 1960 and 1970 (i e, did not change category). Comparing part B with part A, we find generally similar conclusions. A slight decrease in segregation is observed for stable SMSAs in the 500,000 to 1 million category (from an increase of 0.4% to a decrease of 0.2%). A sign reversal also occurs for SMSAs larger than 1 million (from a decrease of 1.2 in the consistent sample to an increase of 0.3 points in the stable growth sample). Overall, the 104 stable SMSAs decreased in mean segregation by 1 2%, while the fast-growing ones, being slightly more segregated, counteracted the trend somewhat. Still, judging from either set of data, we find that declines in residential segregation are quite modest.

Finally, we look at minority proportions using the consistent sample Table 5 shows a curvilinear pattern of mean segregation scores for SMSAs by categories of minority proportion (see pt. A) similar to the earlier findings presented in table 2. Although there is a trend toward increased differences in SMSA segregation between 1960 and 1970 as minority proportion increases, there is an exception in the 20%–30% category. Category changes in minority proportion during the decade do not greatly affect the trends (pt. B). Differences of similar magnitude are observed for both the trends sample and the stable sample.

CONCLUSIONS

Whether American metropolitan areas declined substantially in residential segregation during the sixties is, as we have seen, an open issue. Doubtlessly there were declines from 1960 to 1970 if we consider changes occurring in SMSAs generally. The picture is mixed, however, when we exclude the cities that became metropolitan areas during the decade: overall segrega-

[8] A further complication is the possible impact of annexations in explaining both central-city and SMSA growth At present, the specific effect on residential segregation scores is unknown.

TABLE 5

MINORITY PROPORTION AND TRENDS IN SMSA SEGREGATION 1960 AND 1970

Minority Proportion (%)	1970	1960*	Difference	% Change
		A Consistent Sample		
0.0–4.9	70.8 (32)	75.1 (30)	−4.3	5.7
5.0–9.9	77.1 (41)	77.4 (47)	−0.3	0.4
10.0–19.9	78.8 (39)	77.6 (31)	+1.2	1.5
20.0–29.9	71.3 (18)	72.4 (17)	−1.1	1.5
30+	71.5 (7)	69.2 (12)	+2.3	3.3
Total	75.1 (137)	75.6 (137)	−0.5	0.7
		B Stable Sample		
0.0–4.9	71.0 (24)	74.6 (24)	−3.6	4.8
5.0–9.9	77.5 (35)	77.7 (35)	−0.2	0.3
10.0–19.9	79.4 (31)	77.6 (31)	+1.8	2.3
20.0–29.9	72.7 (13)	73.4 (13)	−0.7	1.0
30+	71.5 (7)	70.1 (7)	+1.4	2.0
Total	75.7 (110)	76.0 (110)	−0.3	0.4

* Percentage nonwhite in 1960 but percentage black in 1970

tion levels changed very little, and although small increases were observed in New England, in parts of the South, and in the middle western states, as well as among the smallest and largest places, they were balanced by substantial decreases in the West.

The findings point to two fundamental conclusions First, generalizations about segregation trends are greatly influenced by characteristics of the cities that investigators study. The results shown here demonstrate that conflicting findings can be attributed, at least in part, to sample variations. Studies that examine segregation in large cities are especially vulnerable to biased generalizations about American cities. The 10 places studied by Clemence (1967) and the 13 studied by Farley and Taeuber (1968) are larger than the mean for the nation, and those authors' claims of increasing segregation during the decade simply do not hold for American SMSAs in all regions, of all sizes, and with varying minority proportions In short, when small, nonrandom samples are used, investigators must use caution in generalizing to national trends.

Second, our results underscore the point that recent improvements in the social and economic status of blacks, and in white attitudes toward blacks, do not necessarily herald large-scale advances in racial integration: segregation levels in American SMSAs remained virtually unchanged between 1960 and 1970. Perhaps a single decade is too brief a span of time to produce noticeable declines in segregation as a consequence of economic improvements and/or attitudinal change; hence, future research should

pursue this issue with more extensive data. Nevertheless, the conclusion seems warranted that residential segregation persists in the seventies and as yet shows little indication of significant decline in response to other racial improvements.

To be sure, there is evidence supporting a continued *gradual* decline in levels of segregation with time. Even this, however, can be questioned, given the existence of major demographic forces at work In recent decades, the nation's metropolitan areas have undergone significant transformations, with continued migrations of blacks to the central cities and whites to the surrounding rings. This massive redistribution has resulted in the increased concentration of the black population in central cities to the point where, in 1970, fully 21% of the total central-city population of the United States was black—almost double the level in 1950 (Taeuber 1974, p. 4). Blacks are now in the majority in Washington, D.C., Newark, and Atlanta; and 12 other cities are at least 40% black. There could easily be a dozen or more major cities with black majorities by the end of this decade Until we can determine more accurately the magnitude and impact of such changes, we can hardly claim that any major shifts in segregation trends have occurred, or are likely to occur, in the near future.

Finally, the focus in the literature (and in the courts) has to date been upon the nation's central cities, to the virtual exclusion of the suburban communities that ring them. There is no systematically analyzed evidence available on levels of suburban segregation—only a few case studies (see, e g , Farley 1976, pp. 28-35) indicating that suburban scores may be higher than those of central cities With the continuation of the demographic changes noted above, and increased pressures for *metropolitan* rather than city school desegregation, future needs are clear. We must turn our efforts away from a narrow concentration on the central city to the broader context of the entire metropolitan area, for it is only thus that the process of residential segregation can be analyzed in its totality.

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Societal Complexity or Production Techniques: Another Look at Udy's Data on the Structure of Work Organizations[1]

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Reanalysis of Udy's data for preindustrial societies shows that production techniques have a stronger direct influence than societal complexity on the structure of work organizations, including skill recruitment, specific goals, achievement, compensatory rewards, and ownership. The effects of societal complexity are equal to the effects of production techniques for only three bureaucratic elements: office rewards, hierarchy, and administrators. However, where workers maintain control of productive factors, even bureaucratic elements are mostly influenced by techniques. These results challenge Udy's thesis that complex peasant societies present more obstacles to the implementation of modern industrial work forms than do more primitive societies.

Stanley Udy (1970) argues that societal complexity markedly affects the structure of preindustrial work organizations. More complex nonindustrial societies, he claims, have less efficient, *socially determined* work organizations that recruit members on the basis of a nonwork role such as a kinship or political role. In more primitive societies, *production-determined* work organizations, which recruit members voluntarily and have more rational structures, predominate. Udy contends that socially determined work organizations of complex peasant societies inhibit the transition to viable industrial work forms (1970, pp. 1–29).

Our recent investigation (Norr and Norr 1974) of tillage and fishing work organizations leads us to question the Udy thesis. Despite their location in highly complex peasant societies where prevailing farming work organizations were socially determined, the fishing work organizations we

[1] This research note is a collaborative effort, and the order of our names does not signify unequal responsibility. The comments of William Bridges, Kathleen S. Crittenden, Phyllis Ewer, John W. C. Johnstone, David Rubinstein, Alan Schnaiberg, and two *AJS* referees are gratefully acknowledged. They are absolved from any responsibility for errors that remain. Computing services were made available by the Computer Center of the University of Illinois at Chicago Circle. Udy's thorough reports (1959*a*, 1970) of his empirical data greatly facilitated our efforts.

examined had all the characteristics of production-determined organizations. Skill and compatibility, not prior social relationships, were the bases for recruitment. Work was limited to economic goals, and there were both less hierarchy and greater specification of the limits of authority in fishing.

Our analysis of fishing and agriculture indicated that technical and environmental constraints affect recruitment and work relations by influencing the relative importance of workers vis-à-vis controllers of capital for effective production. Production techniques required to accomplish particular work goals in a given environment affect the relative power of these two groups by determining the control of strategic resources or the extent to which other opportunities for exchange are limited. Technical and environmental constraints that increase the relative power of workers include exposure to physical risks; uncertainty; separation of work place from residence; difficulty of maintaining clear-cut control of productive factors; and the need for teamwork, skill, and reciprocal coordination. Each of these constraints increases the relative importance of workers for effective production and makes a production-determined structure more likely.[2] Differences between techniques employed in fishing and farming rather than the prevailing social setting determined the structure of work organization.

Udy's earlier analysis (1959*a*) also showed that particular production techniques were associated with distinctive types of work structure. In his later analysis (1970), however, he concentrates on societal complexity and generally ignores the effects of techniques We reanalyze his data and examine the simultaneous impact of societal complexity and production techniques in order to assess the relative effect of each of these factors, a problem Udy leaves unresolved.

DATA, VARIABLES, AND HYPOTHESES

The sample.—For comparisons of production techniques and levels of societal complexity, Udy reports data on 357 work organizations occurring in 125 preindustrial societies sampled to represent world culture areas (Murdock 1957; Udy 1959*a*, pp. 4–9, 139–58; 1970, pp. 30, 130–34).

Societal complexity.—Udy (1970) distinguishes five societal levels in terms of the presence or absence of the following characteristics: (1) exclusive proprietorship, (2) sedentary agriculture, (3) centralized government, and (4) complex stratification. These characteristics constitute a Guttman scale: Level I societies have none of the characteristics; Level II,

[2] For an elaboration of this argument in terms of exchange theory, see Norr and Norr (1974).

only exclusive proprietorship; Level III, exclusive proprietorship and sedentary agriculture; and so on.[3]

Techniques.—Udy's data include seven production techniques: tillage, animal husbandry, construction, manufacturing, collection, fishing, and hunting. These different techniques must be performed within different technical and environmental constraints. Following our analysis of fishing and agriculture (Norr and Norr 1974), we classify these techniques in terms of constraints that limit the structure of work by increasing the relative power of workers: exposure to physical risk; uncertainty; separation of work place from residence; difficulty of maintaining clear-cut control of productive factors; and the need for teamwork, skill, and reciprocal coordination.

Tillage has the lowest levels of the five constraints, usually only relatively little uncertainty and teamwork Collection, fishing, and hunting have the highest levels. Fishing and hunting rank highest on all five constraints; collection sometimes lacks high degrees of physical risk and teamwork Animal husbandry, construction, and manufacturing are intermediate.[4] We examine differences in elements of organizational structure among these three ordered groups to assess the effect of production techniques.[5]

Organizational structure—Four characteristics indicate rationality in work organizations: ***skill recruitment,*** membership determined by skills

[3] We follow Udy in combining Levels I and II for analysis

[4] Udy's discussion of outlay and uncertainty (1959*a*, pp 10–35) and the relationships between production techniques and technical complexity and work load in his data validate our ordering of techniques (There are no codes for outlay and uncertainty, and data on technical complexity exist for fewer than half the organizations) Production techniques with a constant rather than variable work load are more likely to require the integration of a number of complex tasks over a longer span of time and therefore call for greater skill and coordination Tillage is least likely to have a constant work load, collection, fishing, and hunting are most likely, and animal husbandry, construction, and manufacture, intermediate ($\gamma = 0.94$) This order is present at each level of societal development Udy's technical-complexity score primarily measures task differentiation—the number of times the work organization is assembled to complete the entire work process Task differentiation often lessens the need for skill, teamwork, and coordination. There is a negative relationship ($\gamma = -0.70$) between production techniques and technical complexity, again validating our ordering of techniques in terms of constraints The relationship between production techniques and the technical-complexity score is present at all societal levels except the most complex, where there is no significant pattern

[5] The same patterns in relationship among techniques, societal complexity, and organizational characteristics are also found using any of the following groupings of techniques tillage vs. fishing only, tillage vs animal husbandry, collection, construction, and manufacturing vs fishing and hunting; tillage vs all others, and all seven techniques separately Our choice of grouping is based on the theoretical consideration of difference in constraints, the need for sufficient cases for within group analyses, and the desire to avoid an unnecessarily complicated presentation

and availability, not by social affiliation; *specific goals,* organizational objectives limited to material productive ends; *achievement,* rewards to members varying by quantity and quality of work done; and *compensatory rewards,* rewards for participation distributed by higher authority members (Udy 1959*a*, pp. 42–46, 97–105; 1959*b*, p. 793). There are four indicators of the power structure: *worker ownership,* control of factors of production (other than labor) vested in the organization itself and not the manager or an independent individual or group; *office rewards,* rewards differentiated according to office in the organization; *hierarchy,* three or more levels of authority; and *administrators,* some managers not engaged in physical, productive activities (Udy 1959*a*, pp. 2, 36–45, 97–107).[6] Udy designates the last three measures of power structure "bureaucratic" elements of formal organization (1959*b*, p. 793).

Hypotheses.—Udy argues that preindustrial work organizations at higher levels of societal complexity are less efficient and generally unsuited to industrial production: as societal complexity increases, the structure of work organizations becomes less rational and more bureaucratic, presumably for all production techniques We suggest that production techniques incorporating constraints which increase the importance of workers should be positively related to skill recruitment, specific goals, achievement, compensatory rewards, and worker ownership and negatively related to office rewards, hierarchy, and administrators. We expect both the total and direct effects of production techniques on the structural characteristics to be stronger than the effects of societal complexity and to be present at each level of societal complexity.[7]

RESULTS

The effects of production techniques and societal complexity on the structure of work organizations are summarized in table 1. For each

[6] There are missing data for large numbers of cases for the following variables administrators (15%), compensatory rewards (16%), achievement (22%), office rewards (24%), and hierarchy (45%) The proportion of missing data, however, is not associated with either production techniques or societal complexity

[7] To examine fully the relative impact of societal complexity and techniques, data for industrial as well as preindustrial societies are necessary (We focus on fishing in modern societies and the question of the comparative structure of work under postindustrial conditions in Norr and Norr [1975]) In addition, it would be preferable to compare different work organizations in different societies at the same level of development Analysis is hampered also by the absence of detailed and comparable information on all aspects of work relations that we were able to include in our earlier study (Norr and Norr 1974). Nevertheless, the Udy data are the largest, most representative source readily available for work organizations at different levels of societal complexity.

TABLE 1

RELATIVE EFFECTS (γ) OF PRODUCTION TECHNIQUES AND SOCIETAL COMPLEXITY ON THE STRUCTURE OF WORK ORGANIZATIONS

A. EFFECTS OF PRODUCTION TECHNIQUES

			Conditionals within Levels of Societal Complexity			
Characteristics of Organizational Structure	Zero Order	Partial Controlling Societal Complexity	Levels I & II	Level III	Level IV	Level V
Rational elements:						
Skill recruitment	0.83*	0.82*	0.89*	0.87*	0.75*	0.77*
Specific goals	0.83*	0.82*	0.90*	0.87*	0.76*	0.75*
Achievement	0.37*	0.44	1.00	0.36	0.43	0.41
Compensatory rewards	0.33*	0.38	0.17	0.57*	0.27	0.34
Power structure:						
Worker ownership	0.89*	0.87*	0.95*	0.89*	0.84*	0.71*
Bureaucratic elements:						
Office rewards	0.26*	0.40	0.04	0.49*	0.61*	0.00
Hierarchy	−0.40*	−0.25	−0.72*	−0.14*	−0.54*	0.56*
Administrators	−0.50*	−0.26	1.00	−0.07	−0.29	−0.55*
N[a]	(357)	(357)	(60)	(115)	(103)	(79)

B. EFFECTS OF SOCIETAL COMPLEXITY

			Conditionals within Groups of Production Techniques		
Characteristics of Organizational Structure	Zero Order	Partial Controlling Production Techniques	Tillage	Animal Husbandry, Construction, Manufacture	Collection, Fishing, Hunting
Rational elements:					
Skill recruitment	−0.41*	−0.11	0.12	0.34	−0.32
Specific goals	−0.38*	−0.06	0.25	0.42	−0.31
Achievement	0.03	0.18	0.17	0.59*	0.07
Compensatory rewards	−0.16	−0.06	−0.20	−0.06	0.25
Power structure:					
Worker ownership	−0.63*	−0.42	−0.05	−0.16	−0.59*
Bureaucratic elements:					
Office rewards	0.21	0.40*	0.48*	−0.07	0.51*
Hierarchy	0.42*	0.33	0.28	0.24	0.44
Administrators	0.62*	0.54*	0.59*	0.70*	0.32
N[a]	(357)	(357)	(144)	(87)	(126)

SOURCE: Udy 1959*a*, 1970.
[a] *N*s are total number of cases in the category; see n. 6 on missing data.
* Significant at the $p < .05$ level.

organizational characteristic, zero-order γ measures the total effect of each variable, production techniques and societal level. The ordered grouping of production techniques and societal complexity are negatively related, $\gamma = -0.50$. Partial γ indicates the direct effect of each variable, with the effect of the other variable controlled. Conditional γ's show the effect of

each variable within each of the values of the other variable to test for interaction effects.[8]

The results for the four rational elements—skill recruitment, specific goals, achievement, and compensatory rewards—are similar and support our hypotheses. The zero-order relationships are in the predicted direction, positive for production techniques and negative for societal complexity. (The only exception is the relationship of societal complexity with achievement.) Both the total effects and the direct effects of production techniques are very much stronger than those of societal complexity. Positive relationships between rational elements of structure in work organizations and production techniques incorporating constraints that increase the importance of workers are present at all levels of societal complexity. But there is substantial interaction for societal complexity. The consequences of societal complexity are not the same within each group of production techniques: societal level has effects in different directions on different rational elements of work structure for different groups of techniques.

Results for measures of power structure show more variation. Worker ownership parallels the rational elements: production techniques have a stronger impact than societal complexity, the positive association with techniques is present at all levels of societal complexity, and the effect of complexity varies among groups of techniques.

The three bureaucratic elements of power structure—office rewards, hierarchy, and administrators—are less consistent with our hypotheses. Five of the six zero-order associations are in the predicted direction; only that between office rewards and production techniques is in the opposite direction. For the total sample of organizations, however, production techniques do not show greater effects than does societal complexity Rather, their influences are approximately equal. The effect of techniques for bureaucratic elements at different societal levels is not so consistent as for rational elements or for worker ownership, and the effects of societal complexity within different groups of production techniques are more consistent.

Worker ownership plays an important role in conditioning the impact of production techniques and societal complexity of bureaucratic elements of power structure. Where workers instead of a manager or separate group have control, results show considerable support for our hypotheses. Limiting analysis to corporately owned organizations reverses the relationship of production techniques with office rewards to the predicted negative

[8] The choice of γ for the statistical measure of association is suggested by its appropriateness to the level of measurement of the variables, its greater familiarity among readers, and its use by Udy (in the form of Yule's Q) in his earlier work (1959*a*) Kendall's τ C and Sommer's D (symmetric) and Pearson product-moment correlations all show the same pattern of relationships as γ

direction (—0.20). Also the effects of production techniques on hierarchy and administrators increase, and those of societal complexity decrease, to the point where they are similar to the patterns for the rational elements (zero-order γ's: hierarchy = —0.70 for production techniques vs. 0.39 for societal complexity; administrators = —0.71 for production techniques vs. 0.07 for societal complexity; partial γ's: —0.80 vs. 0.41 and —0.65 vs. 0.38, respectively).

Overall, these results confirm our predictions and support the conclusions of our earlier analysis (Norr and Norr 1974). Production techniques have a stronger impact on the structure of work organizations than does societal complexity. For most organizational characteristics, the zero-order and partial measures of association are larger for production techniques. For the bureaucratic elements where this is not the case, the relative impacts are equal, and the predicted relationships are present where workers control the organizations There is much greater inconsistency in the impact of societal complexity Frequently, the direction of the relationship is different among the groups of production techniques.

DISCUSSION

What seems to be important in the determination of work relations in organizations is the relative dependence of those who control capital and those who labor. Particular techniques necessary to accomplish work and characteristics of the environment affect the relative power of these two groups by determining control of strategic resources and the extent to which other opportunities for exchange are limited. The greater the need for close coordination, flexibility, and mutual feedback among workers and the fewer the tasks subject to set rules and planning, the greater the dependence on and, therefore, the power of workers.[9] Such situations are more likely when the environment is unpredictable, raw material is not uniform, or the process of transformation is not well understood (Perrow 1967; Thompson 1967; Woodward 1965). Inability to maintain clear-cut control of production factors, physical risks, and separation of work from residence also increase the importance of workers directly and indirectly, by increasing the need for skill, teamwork, and coordination.

Where workers are relatively important, work organizations are more rational, less bureaucratic, and more controlled by workers. Societal complexity appears to have little effect on rationality in work organizations. Its impact is limited to bureaucratic elements of power structure. At

[9] The argument here is similar to Stinchcombe's (1959) explanation for the persistence of a nonbureaucratic, craft organization in construction in response to relatively constant technical and environmental constraints

higher levels of development, it becomes easier to institute bureaucratic controls that counter the effects of techniques. Greater societal complexity tends to mean higher levels of capitalization and a corresponding increase in the importance of capital and management relative to labor. Workers lose control of work organizations and become a market commodity (Braverman 1974). If workers can manage to keep control of work organizations in the face of higher levels of capitalization, the imposition of bureaucratic features accompanying societal complexity can be resisted.[10]

In large measure, the effect of societal level reflects a changing distribution of techniques. The move from the primitive to the complex peasant societal level involves an increasing proportion of tillage organizations and a decreasing proportion of other techniques. Many of the differences in work organizations among societal levels are accounted for by differences between tillage and other techniques and are not the result of changes in the social setting of work.

These results raise questions about Udy's conclusions that the conditions of work in peasant-level societies are unfavorable for the development of viable, modern work organizations It is tillage that is organized unfavorably for industrial work Nontillage work organizations in complex peasant societies are successfully insulated from the patterns of organization institutionalized in predominant agricultural activities and are affected primarily by the demands of their own techniques.[11] Industrial activities should be at least as insulated, if not more, and at least as responsive to technical and environmental constraints as are traditional nonfarming activities.

In most industrial societies, tillage organizations have increased in acreage and level of capitalization, while the number of persons involved and the importance of tillage in the total economy have decreased. But the socially determined structure of preindustrial tillage has been retained. Even in planned economies, attempts to change agricultural work organizations have met with considerable resistance, except when introduced to previously landless peasants. One might suggest that socially determined work organization is an effective and efficient way of meeting the environmental and technical constraints of tillage.

We do not underestimate the problems of creating new organizations under the best of circumstances. Creating modern work organizations in all preindustrial societies is complicated by the absence of supportive infra-

[10] Our analysis of modern fishing (Norr and Norr 1975) shows that intervention by outside agencies such as governments or cooperatives can prevent the loss of control of fishing to nonfishermen and the consequent increase in bureaucratization and relative deprivation of fishing workers.

[11] See Norr and Norr 1974, pp. 243–45, on the forms of integrating divergent work norms in complex peasant societies.

structures such as general literacy and a differentiated education system. We do not see evidence, however, to support the implication of Udy's thesis that prevailing work patterns make industrialization in complex peasant societies *more* difficult than in primitive societies. Nor would historical and comparative analysis provide support. Industrialization has thus far emerged only out of complex peasant societies, and the more industrialized of the Third World societies today such as Mexico or Indonesia are peasant—not primitive.

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Commentary and Debate

ON LIN AND YAUGER'S CROSS-NATIONAL COMPARISON OF THE PROCESS OF OCCUPATIONAL STATUS ACHIEVEMENT

As Lin and Yauger (1975) note, comparative studies of status-attainment processes involving data from less developed countries (LDCs) are badly needed. While we think that they were correct in attempting to fill this void, we take serious objection to some of the data they present, and we question the tenability of their conclusions.

Briefly, what they tried to do was to estimate national status-attainment parameters for two LDCs (Costa Rica and Haiti) and two more highly developed countries (MDCs: the United States and Great Britain). So far as we can tell, the data for the MDCs (Treiman and Terrell 1975) are appropriate. It is the adequacy of the LDC data which we doubt. Our misgivings pertain to three general problems: (1) rural sampling bias, (2) high nonresponse rates, and (3) occupational status scoring of low validity.

1. Rural sampling bias. Both of the LDC samples were drawn from rural areas of the respective countries. Surely a disproportionate number of the better educated, more prestigious, and more influential people in Costa Rica and Haiti live in the cities. So the higher reaches of the status system were almost certainly undersampled. Clearly, the only appropriate way to obtain national level status-attainment parameters is to base them upon representative national samples. There seems to be no way by which the parameters estimated from these two samples can legitimately be considered valid estimates of their respective national parameters It follows that the comparisons drawn with the MDCs are untrustworthy.

2. High nonresponse rates. Compounding the first problem, in both LDC samples many cases were dropped from the analysis because of missing data. The intergenerational mobility analysis for Haiti was based upon only 37% of the original sample (199/544). For Costa Rica the corresponding figure is 59% (337/570). In Lin and Yauger's regression analysis the percentages are 86% (455/544) for Haiti and 59% (336/570) for Costa Rica. It is difficult if not impossible to assess the error in the estimates of the parameters which is due to such underrepresentation But surely, even if they had used adequate national sampling frames, nonresponse rates as high as some of the above would make their estimates dubious.

3. Validity of occupational status scores. Both the nominal occupational codings and the numerical scores assigned to them by the SIOP (Standard Index of Occupational Prestige; Treiman, forthcoming) are probably inaccurate, for two reasons. First, from our rural Brazilian work (e.g., Haller,

Holsinger, and Saraiva 1972) we have come to believe that agricultural occupational titles are not part of the urban vocabulary in Latin America. Urban interviewers and coders, consequently, are often unable to interpret accurately the meanings of the words agricultural respondents use to describe their job titles. Unable to distinguish among farm occupations, they label most farm workers with some blanket term like "day laborer" or "skilled worker," thus losing important nominal distinctions It is hard to imagine that the proper prestige scores could be assigned to improperly classified occupational titles. We suspect that this makes the occupational prestige scores less valid than is required for careful comparative research. Second, we would suppose that some of the farmers, especially those in the smaller farm areas near San Jose, Costa Rica, might have had other jobs. How are prestige scores assigned to such people? Were their other jobs more or less important to them than farming? Were they more or less prestigious than farming? There is no way one can tell from the article. Surely, an unknown amount of invalidity must have been introduced in the scoring of occupational prestige as a consequence of multiple job holdings among farmers. Both of these factors thus should reduce the validity of the occupational prestige scores.

The principal adverse consequence of these three fundamental shortcomings (biased samples, missing data, and low validity of the prestige scores) is an almost certain deflation of the values of the status parameters estimated for the LDCs Obviously, this would tend to exaggerate the differences between them and the MDCs.

Our reasons for being skeptical of the data from the LDCs and the authors' interpretation of them are not wholly speculative. In our own Costa Rica study (here called the Turrialba Longitudinal Study, or TLS) of men first interviewed as high school students (all first- to fourth-year students in school on the day of the testing) in 1959 and later reinterviewed in 1968, we obtained quite different findings (Hansen and Haller 1973). Most important, we found the path from education to occupational prestige to be $\beta = .56$, which is very close to that found in similar research in Wisconsin ($\beta = .52$) and in the United States as a whole, in the data set used by Treiman and Terrell and by Lin and Yauger ($\beta = .53$). Of course, ours is no better a sample by which to estimate the corresponding Costa Rican parameter value than that employed by Lin and Yauger. But that is not the point. This datum illustrates forcefully the well-known fact that nonrepresentative samples such as theirs and ours provide poor statistics by which to estimate population parameters.

Turning to their criticism of our study, Lin and Yauger, we fear, have glossed over the TLS data. (*a*) They fail to point out that the TLS was a long-term project, not merely a study of "enrollees in a high school in a county in Costa Rica in 1959." The sample members were reinterviewed

nearly a decade later (1968). (*b*) They overemphasize the smallness of our TLS sample size (103), which in fact is quite close to two of their own three Costa Rica subsamples ($N = 103, 104, 221$) whose statistical differences they take quite seriously. (*c*) Although they are correct in saying that the TLS men were "privileged," the privilege consisted mostly in living near a high school. It certainly does not mean that they were from high-status families, as Lin and Yauger imply In fact, the appendix to our article shows quite clearly that their fathers and mothers were rather poorly educated (averaging about two years each) and that most of their fathers were blue-collar and lower-white-collar workers. (*d*) Lin and Yauger's point about the homogeneity of our sample is true but misleading. Certainly the sample was not homogeneous with regard to the sons' status variables. The means and standard deviations for our respondents' education and socioeconomic index (SEI) were 11.46 ± 2.20 years of schooling completed and 55.42 ± 19.44 SEI points, which is about as heterogeneous as we find in American cohorts. Regarding age, the sample was obviously quite homogeneous inasmuch as it was a high school student cohort. In 1959 their mean age was 16.28 ± 1.50 years In 1968 they were, of course, nine years older. If our sample had been homogeneous with respect to the educational and occupational statuses of these men, it would have been impossible to obtain such a high β as we did. (*e*) Finally, their criticism of our use of Duncan's (1961) SEI is erroneous. The SEI is a well-known expansion upon the NORC prestige scores available during the 1950s and 1960s. It appears to provide an almost identical ranking of the same occupations and provides a rational assignment of scores to many others for a number of populations As has been demonstrated (Haller 1972), there are rural people for whom urban occupational prestige scores such as the NORC scores, the SEI, and possibly the SIOP might not be fully appropriate. But the men in our Costa Rican sample are not among them. In fact, for this particular sample, the correlation between the NORC ratings of 80 occupations and those provided by our respondents while they were still in school in 1959 is $r = .87$ (Haller and Lewis 1966). So it turns out that SEI scores are satisfactory for the TLS sample The criticism of the SEI leveled by Lin and Yauger does not merit extended consideration. Their argument seems to be that because the SEI is partly based on aggregate education data there will be a high autocorrelation between SEI scores and education at the individual level. This is untrue, as has been known for many years (Robinson 1950).

Highlighting the already glaring faults in their analysis, our findings question the credibility of their conclusions about status attainment in Costa Rica. If their Costa Rican analysis—to which they devote a disproportionate amount of attention—is spurious, what about Haiti? There is no reason to believe that the sampling and scoring problems in their Hai-

tian data are less severe than those in their Costa Rican data. Finally, if their analysis of status attainment in the LDCs is unsound, the conclusions they draw from their comparisons of parameters estimated from these samples with those of Britain and the United States are also unsound.

In sum, there is no doubt about the need for comparative status-attainment research. However, in such studies extreme caution should be taken in assessing sampling imperfections and data reliability and validity. Lin and Yauger appear to have used grossly unrepresentative LDC samples and somewhat questionable measures to estimate and compare national parameters. This is not the way to advance comparative knowledge of status-attainment processes.

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REPLY TO HANSEN AND HALLER

The arguments presented in the Hansen and Haller comment address two issues treated in our article: the sampling and measurement problems of the data gathered in Costa Rica and Haiti (Lin and Yauger 1975) and the comments made in the article relative to a data set gathered by them in Costa Rica (Hansen and Haller 1973). We shall respond specifically to

the points raised and, in the process, make some general observations about research in the area of stratification and development and the process of validation.

Hansen and Haller question strongly the validity of our conclusions on three grounds: (1) rural sampling bias, (2) high nonresponse rates, and (3) problems of measuring occupational status. They concluded that these problems led to deflation of the values of the status estimates.

1. Regarding the rural samples we used, the objection is that rural samples do not constitute national samples, which should include urban representations, and are therefore "untrustworthy" for inference to the national parameters. In the article, we never set as our goal to "estimate national status-attainment parameters." In fact, we never used the term, "national parameters" in the article. On the other hand, we emphasized in the section on sampling and data collection and in our concluding remarks that our data were from rural areas, and we urged that in future research efforts "samples should be drawn in such a way as to contain respondents from both rural and urban populations in order to reflect the distribution of the national population" (p. 558)

The more crucial question, implicit in their objection, is whether data are invalid because certain segments of the population are explicitly excluded It is not unusual for studies of mobility to exclude specific sectors of the population (such as the farm sector, certain age sectors, and the nonwhite sector in the United States and elsewhere) and for the analysis and interpretation therefore to be restricted to the major populations of the countries involved (Treiman and Terrell 1975, p 566) Since the exclusion is made explicitly and conceptually, and the majority of the population in the countries studied constitute the parameters for estimation, we see no reason why inference from the sample to the population specified is "invalid." Hansen and Haller do not raise such an issue concerning the American and British data used in our article. Yet they object with regard to the majority populations of the two less developed countries (only 37% of Costa Ricans and 17% of Haitians lived in urban areas in 1970 [Agency for International Development 1971]).

Theoretically, the issue of urban-rural distinction takes on further significance in attempts to model the relationships between development and status attainment. One does not have to be reminded of the fact that over 60% of the world population still live in rural areas (Davis 1972) in order to conceptualize the importance of inclusion of the rural samples in the mapping of the status-attainment process as it is related to levels of development. Truncation of the rural samples would eliminate the values on the development continuum in such a way as to distort any theoretical propositions explaining the relationships between development and the status-attainment process.

2. The issue on the nonresponse rate certainly is important. However, there are two considerations in this regard. One concerns the loss of respondents in the original sampling effort; the other, the elimination of missing observations or respondents excluded from the analysis for specified conceptual or methodological reasons. Since the loss of respondents from our original Costa Rican and Haitian samples was extremely low (about 10%–13%; see Lin and Yauger [1975, pp 545–46]), we assume that Hansen and Haller object mainly to the reduction of observations. Again, they ignored the comparative information regarding the U.S. and British data used. While 86% of our Haitian respondents and 59% of our Costa Rican respondents were included for analysis, only 59% (536/908) of the British respondents and 51% (10,479/20,700) of the U.S. respondents were retained in the path estimations (Treiman and Terrell 1975). The ratio of retained observations over the total sample, therefore, showed no evidence that the Costa Rican and Haitian data suffered high loss. This is not to say that one should not pay attention to the eventual elimination of potential missing observations in one's original sample design. But, there is no evidence to suggest that our data from Costa Rica and Haiti were abnormally biased because of the procedure of eliminating unqualified observations from the analysis.

3. The issue of interviewers and coding schemes regarding the occupational status scores was also raised Our interviewers were ones who either worked or lived in the rural areas studied, not "urban interviewers" as Hansen and Haller thought. Further, we attempted to gather sufficient information to differentiate farm workers. As indicated in the appendix of the article, there were three groups of "day labor," depending on seasonal or constant commitment and invariance of work place, and four groups of farmers or "agriculture " We are at a loss as to why Hansen and Haller attributed their own experience and observation of urban interviewers labeling "most farm workers with some blanket term like 'day laborer' or 'skilled worker' " to our interviewers and coding schemes.

We agree that jobs other than full-time or major jobs should be investigated. We did examine all jobs held by respondents and their spouses in Haiti and found the part-time jobs to be uniformly equal or lower in occupational status than the primary job status We did not examine part-time jobs in Costa Rica; therefore, no data could be provided to test their speculation that the part-time jobs may or may not be more prestigious than farming. There is no evidence in the literature to suggest that part-time jobs are higher in prestige than primary jobs. It is a legitimate question for investigation. But, at the same time, unless some evidence exists for suspecting higher prestige scores for part-time jobs, one cannot be held responsible for testing every speculation raised.

"The principal adverse consequence," Hansen and Haller conclude, was

"an almost certain deflation of the values of the status parameters estimated." If they had read our article carefully, they would have noticed that their speculative conclusion was false. The Costa Rican data showed, in aggregation (table 2) and in three subgroups (table 4), that the path coefficients from father's occupational status to son's occupational status ($b_{fs\,c}$), were uniformly and consistently higher than those obtained in Britain and the United States. If one takes these results and Hansen and Haller's deductions seriously, then one is led to conclude that their objections to the validity of the data were unfounded.

As to the Costa Rican data reported by them in their 1973 article, our comments on the shortcomings stand as they were stated in the footnote of our article. Here, we wish only to address two issues discussed in detail in their comment. the use of Duncan's socioeconomic scale of occupational prestige and the similarity of the magnitude of the path coefficient from education to occupational prestige they obtained in their data to that found in U S. data.

Our concern about the use of the Duncan SES scale by Hansen and Haller was not addressed to its validity but, rather, to its comparability with the Treiman scale used in the U S , British, Costa Rican, and Haitian data as reported in our article. While it has been found that the correlations among the various occupational status scales range from 80 to .90, it is also found that the coefficient between education and occupational status as measured with the SES scale is consistently higher than that when the occupational status was measured with the NORC or the Treiman scale (Treiman and Terrell 1972, Duncan, Featherman, and Duncan 1972). Contrary to Hansen and Haller's statement, Robinson (1950) in fact showed that ecological correlations based on collapsed categories are higher than individual correlations under the usual circumstances when the within-collapsed-category correlation is less than the total individual-category correlation (p. 356, eq. 3) As the number of categories collapsed increases or as the within-collapsed-category correlation increases, the ecological correlation descends toward the individual correlation. Since the SES scale was constructed on 45 occupational categories, a sufficiently large number (Duncan 1961), it is not surprising that the scale produces consistently and slightly higher coefficients than one produced on individual categories Thus, for this and other reasons, we felt it would be misleading to incorporate their coefficients in our graph, as urged by one anonymous *AJS* referee who read our manuscript.

We find it surprising that Hansen and Haller would consider a similarity between a path coefficient pertaining to a privileged group of respondents in a less developed country and one pertaining to the United States as the basis of validating data from the less developed countries. Are they suggesting that data collected elsewhere must be validated against U.S. find-

ings? How would they justify the fact that both British and Australian data yield path coefficients from education to occupational prestige lower than that yielded by the U.S. data ($b_{f,N}$) is .39 in Britain as reported by Treiman and Terrell, and also in Australia as reported by Jones and recalculated in our article, as compared with .50–.53 reported in U.S. data)? Are the British and Australian data invalid also? Or, is there perhaps a theoretical explanation for "a looser fit between the educational and occupational systems" (Jones 1971, p. 535) as one goes from the more developed to the less developed areas? Efforts in theory (Treiman 1970) and our own research efforts clearly show the latter is a distinct and meaningful possibility.

When data are gathered from a privileged sample of respondents in a less developed country, as was done by Hansen and Haller in Costa Rica (the average schooling of their respondents paralleled the U.S. average [12 years] while the national average was less than four years and less than 30% of the school-age children enrolled in secondary schools in 1960 [UNESCO 1969]), should the resulting data be used in any way as the basis of validation of other data collected in that country representing the majority population education level? Do Hansen and Haller really believe that the privilege of going to secondary school and beyond in a less developed country or any country where attendance in secondary school is not mandatory is due mostly to "living near a high school"?

Validation is a rigorous process all sociologists must pursue in spite of the incompleteness of the data we gather. Both conceptual and empirical confirmations must be examined and resulting data be interpreted in cautious terms. However, we do not believe ideological validation should be part of our effort.

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COMMENT ON ZALD'S REVIEW OF *THE WELFARE STATE AND EQUALITY*

In his long review of my short book, *The Welfare State and Equality: Structural and Ideological Roots of Public Expenditures* (*AJS* 81 [January 1976]: 942–45), Mayer Zald says that he finds it provocative, exciting, and disappointing. I would like to ease his disappointment by clarifying his vague theoretical disagreements, which he presents in the guise of technical objections, and by correcting plain distortions of what I say Zald asserts that my alleged "errors in the causal modeling may well lead people to ignore . . . a highly provocative and stimulating set of hypotheses, observations, and interpretations"—and, by riveting his attention on about five pages of the book, manages to obscure its substance.

Zald begins by claiming that technical errors presumably cast doubt on my conclusion that a country's social security spending is importantly shaped by the fraction of old people in the population. I leave it to readers who can work their way past his review to judge the merits of his criticism of pages 22–27 of my book. At best his assertions are controversial; at worst they are statistical misconceptions. But no matter. For Zald goes on to argue that my conclusion about the power of the aged in welfare-state development is either (1) obvious or (2) wrong because "in fact, family and child assistance, public health services and insurance, and unemployment subsidies are also large parts of social security expenditures." Not once but several times (pp. 2–3, 26, 90) I state that the aged are the main beneficiaries of the two program packages comprising the welfare state that are by *far* the most expensive—pensions and health insurance. Even in countries with liberal benefits and considerable unemployment, unemploy-

ment benefits and related labor market policies constitute a trivial fraction of GNP.

In the five pages Zald singles out, as well as in other places where I mention the aged, my main aim is to constrain my own enthusiasm for subtle structural explanations and to warn other students of modern society not to spin a web of theory about structure and change that fails to incorporate the necessary demographic forces

"Most important," Zald writes, "the variable percentage of aged is a surrogate variable for health status, urbanization, effective birth control, and modernization, as well as aging . This variable has much too much theoretical ambiguity. . . . [It] may be a pure demand variable." He also claims that I do "not argue that the aged are a pressure group "

The theoretical meaning of this variable is clearly stated in the paragraph on page 26 which begins with the sentence "Old people . constitute a . . . political force." On the same page I relate effective birth control (i e., declining birthrates) to both economic growth and an aging population. The context of my argument is summarized on pages xiii, 26–27, 47–49, and 86: "Whatever their economic or political system, whatever the ideologies of elites or masses, the rich countries converge in types of health and welfare programs, in increasingly comprehensive coverage, and, to a lesser extent, in methods of financing The fraction of national resources devoted to these programs climbs, eventually at a decelerating rate" (p 86) The root cause of the general trend is economic growth and its demographic and bureaucratic outcomes. Thus, economic growth has produced a large proportion of old people--at once "a population in need and a political force for further social security development." The main reason for this demographic trend is a marked decline in birthrates. "Leaving migration aside and assuming that death rates do not increase, every day the birthrate declines, the proportion of people aged 65 and over creeps up . Big differences in past trends in birthrates have produced sharp contrasts in the present proportion of aged by level of economic development" (p. 26).

Finally, the ritual invocation of such loose concepts as "urbanization" and "modernization" to explain everything in sight is the plague of textbook sociology. I left urbanization out of my explanation because of the well-known "overurbanization" of poor countries with young populations. If one controls for GNP per capita, age of population, and age of system, urbanization washes out completely as a predictor of welfare effort Data on the "industrial labor force" (mining, manufacturing, transportation, communication, other?), which Zald says I overlook, are not only *very* shaky for 60 countries, but are no more than a clue to urbanization. The more concrete and relevant attribute of structure, "size and strength of the working class," is discussed on pages 65–67 and, as I indicate there, we are in the process of measuring this for rich countries where the data

are passably comparable. As for "modernization," it cannot be defined clearly except in terms I have already included in my study.

Several times Zald charges me with ignoring political processes or variables. Aside from analysis of the welfare backlash and ideological themes in chapter 2, I offer in chapters 3 and 4 the following hypotheses to explain the differences among rich countries between "welfare state leaders" and "laggards" matched for economic level: the welfare state is most developed and supporting ideologies are most powerful where a centralized government is able to mobilize and must respond to a large, strongly oriented working class with only medium rates of social mobility, where the "middle mass" (upper working class and lower middle class) does not perceive its tax burden as grossly unfair relative to that of the rich and upper middle class and does not feel great social distance from the poor, where the tax system has low visibility (e g , least reliance on direct income or property taxes), the private welfare state is limited, and the military establishment modest. Even a casual reader would be unlikely to miss the place of political forces in that explanation.

By an odd device, Zald disposes of my final chapter, "The Impact of the Welfare State on Real Welfare": he praises two other scholars whose relevant work I summarize accurately and appreciatively and berates me for *not* citing their *ir*relevant work. Pryor's excellent *Property and Industrial Organization in Communist and Capitalist Nations* has nothing to do with the subject of my book, although it does bear on my larger purpose—to test convergence theory. Cutright's careful work on the determinants of social security spending is discussed where relevant. In the work Zald mentions, Cutright does not deal with the impact of public spending on income distribution as Zald claims; he simply assumes that social security spending as a fraction of GNP is a measure of income redistribution efforts in 40 countries. In contrast, I take pains to demonstrate that, if we consider only the short run and examine good data on the payout of benefits in cash and kind as well as the incidence of all taxes and contributions for two countries with contrasting regressivity in their financing of social programs—UK and West Germany—we find that the net effect is egalitarian By using the example of health services, I then suggest the difficulty of applying that short-run statement to the long pull: "The poor die young—before they can contract the chronic diseases that dearly cost national health schemes. The more affluent citizens live to a riper age, chronically collecting health services paid for by the lifelong taxes of the deceased poor. A program that is highly progressive at a cross-sectional moment may be highly regressive in the lifetime of particular generations" (p. 96).

I deplore the tendency in sociological book reviewing toward uncritical applause; I welcome Zald's willingness to mention the topics in my book and his effort to assess evidence critically. Unfortunately, his obsession

with overelaborate and misleading technical arguments trivializes and distorts the sociological substance of my work.

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REPLY TO WILENSKY

Professor Wilensky's comment is like his book: It has some points that may be well taken, but it is marred by errors and undocumented or poorly documented assertions.

Three minor points:

1. Pryor's work is not as irrelevant as Wilensky states: it has a substantial section on the determinants of wage inequality More important, I *did not say* that he should have cited Pryor's book.

2. Cutright's 1967 article may be only partially relevant to Wilensky's chapter 5 (when writing it Cutright thought it was relevant to the topics that Wilensky treats there); but it is extraordinarily relevant to the book as a whole and to the statistical materials in chapter 2. After all, it utilizes similar indicators for many of the same theoretical constructs, and its dependent variable, social security expenditures in relation to GNP, is the same as employed by Wilensky.

3. I did misstate myself about Wilensky's interpretation of the role of aged But my general point is still well taken Percentage of the population over 65 years old in 1966 is not a good political indicator for age of the social security system in 1966. Nor would it be the best measure of "political force" at the time that social security systems were enacted. What is at stake is the relation of theoretical concepts to indicators

But the major problem with Wilensky's reply is that it trivializes my criticisms (1) Readers should not be misled by his implication that my criticism applies only to pages 22–27 of his book. There are two other path models in chapter 2, and my criticisms apply, in varying degrees, to these as well. The path models are a major evidential base for the argument in chapter 2. Thus, in my opinion, the conclusions drawn from the statistical analyses in this chapter are suspect (The chapter constitutes 30% of this short book.) (2) Wilensky states: "At best his [Zald's] assertions are controversial; at worst they are statistical misconceptions But no matter." No support for those sentences is given. I do not believe he can so cavalierly ignore problems of multicollinearity (Gordon 1968), or the use of ratio variables with common terms (Schuessler 1973) There *are* debates about how one minimizes error in this situation; but, so far as I can ascer-

tain, there is *no* debate that both problems introduce error into statistical analysis.

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Book Reviews

Kinship in Bali. By Hildred Geertz and Clifford Geertz. Chicago: University of Chicago Press, 1975. Pp. xiv+213. $12.50.

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In conventional anthropological usage, "the kinship system" of a society consists in the totality of social relations which people enter into as kinsmen of one another This system includes, of course, the ideas, beliefs, and values they deem relevant to their social relations as kinsmen, especially the ideas by which they define themselves as "kinsmen." No one maintains that this system as a whole is anything but an analytical abstraction or that it is in any sense autonomous in relation to other analytical systems—such as political, economic, and religious systems—which we find it expedient to recognize Indeed, ethnographers of the functional persuasion have yet to tire of reminding us that kinship cannot be studied sociologically as a thing in itself, because the social and sociological significance of kinship rests on the fact that it serves as a criterion for the allocation of rights and duties or privileges and obligations, whose content may be political, economic, religious, or whatever. From this perspective it is only tautological to say that any sociology of kinship must take into account matters of politics, economics, religion, and the like Further, it is widely recognized that concepts about the very nature of kinship (genealogical connection) are sometimes closely related to other, especially religious and metaphysical, concepts (see, for example, M Fortes, *Kinship and The Social Order* [Chicago· Aldine, 1969], pp 101–21)

It is therefore somewhat puzzling to find the Geertzes insisting that, to the contrary, anthropologists typically treat kinship as an "autonomous system" (of sentiments, norms, or cultural categories) and proposing the "radical" notion that kinship is best regarded as a variety of "cultural idiom" which "is not an autonomous system at all, but an integral part of a more inclusive 'idiom' or 'culture pattern' . . ." (pp. 2–3, 153–58). Here the Geertzes join company with several other anthropologists who in recent years have devoted much effort and not a little polemic to misguided and futile attempts to argue the descriptive-analytical categories "kinship" and "kinship system" out of existence, or at least to curtail drastically the range of their applicability. In one form the claim is that culturally posited relations of genealogical connection are not at all relevant to the definitions of certain social identities or cultural categories hitherto described as kin categories In another form the claim is that, although culturally posited relations of genealogical connection are relevant to the definitions of certain social identities or categories, other kinds of relationships also are relevant; therefore the identities in question cannot be described unambiguously as kin categories; they are also, by cultural

definition, categories of other kinds. From this perspective, that of the Geertzes in *Kinship in Bali,* the integration of kinship with other institutions is not merely structural-functional; it is, perhaps primarily, cultural, or in other words, at the level of symbols and their meanings.

The Geertzes begin by distinguishing between the "cultural" and "social structural dimensions" of "Balinese kinship customs and practices" (p. 2). "By 'the cultural dimension'," they say, "we refer to those Balinese ideas, beliefs, and values which are relevant to their behavior as kinsmen. . . ." Then, as if to specify how the Balinese themselves recognize one another as kin or nonkin, the Geertzes add: "The relevant ideas, beliefs, and values are those having to do with, for instance, the perceived nature of the connection between parent and child, or between deceased ancestors and living persons, or between individuals who share (or think they share) a common parentage or common ancestry."

Were it not for the qualifying "for instance" and the vagueness of the phrase "the perceived nature of the connection," this would seem to imply that, for the Balinese, as for anyone else, "kinship" is essentially a matter of genealogical connection--to be genealogically related to someone is to be his or her kinsman The Balinese may, of course, have some more or less distinctive ideas, beliefs, and values about the significance of being a kinsman or a particular kind of kinsman; and it may well be, as the Geertzes go on to say (p. 3), that Balinese "conceptions of kinship [are] integrated with even more fundamental conceptions, stemming especially from the realms of religion, residence, and social rank, into a comprehensive cultural pattern." None of this would contradict the propositions that to be genealogically related to someone is to be his or her kinsman, and that kinship is, by cultural definition, a matter of genealogical connection

If these propositions were accepted, it could be argued that kinship in Bali differs from kinship elsewhere not in its culturally definitive attributes but only in its contingent features---that is, in those features which constitute the "meaning" or cultural significance which Balinese attribute to being a kinsman or a particular kind of kinsman. There would be no inherent contradiction in describing these features as "contingent" and, at the same time, insisting that they be taken into account if we are to understand the "meaning" of kinship in Bali. For how are we to understand the expression "the 'meaning' of kinship in Bali" except as referring to the ideas, beliefs, and values which constitute the culturally constructed significance of being a kinsman or a particular kind of kinsman? And how are we to identify such ideas, beliefs, and values, except by reference to some Balinese concept whose designation may be sensibly glossed as "kinsman" or "relative" because its distinctive feature is relationship by genealogical connection to an ego or propositus?

But the Geertzes do deny, or at least seem to deny, these propositions. With no more than one passing reference to, and no further explication of, one Balinese expression they gloss as "kinsmen" ("*njama,* 'siblings, kinsmen'," p. 52), they quickly focus their attention on the *dadia,* a corporate aggregation of agnatic extended families. In the process, they virtually

equate the problem of understanding kinship in Bali with the problem of accounting for the occasional and irregular way in which kinship obtrudes from the "domestic" and enters the "public" domain as sets of families gradually become economic and political factions and then incorporate—by moving their joint family shrine from one of their "houseyards" out into public, hamlet land. Also in the process, the Balinese "kinship idiom," at first defined as the totality of ideas, beliefs, and values relevant to Balinese behavior *as kinsmen,* is narrowed down to "the Balinese '*kawitan* [origin point] and *dadia*' idiom." Eventually it is claimed that "whatever system there is in all this . . . it is not likely to be formulable in terms of the sentiments, the norms, or the categories pertaining to 'consanguinity and affinity.'" Indeed, the Geertzes continue, these are "notions as foreign to the Balinese '*kawitan* and *dadia*' idiom as those are to ours" (p 155).

This, it would seem, is best understood as a rhetorical overstatement of an otherwise valid point; the incorporation of sets of agnatic extended families must be understood as a political-economic and even religious act, and not as a process governed wholly and solely by sentiments, norms, and categories of kinship, disembodied— if such a thing is even imaginable— from their political, economic, and religious content But if the process requires a political-economic-religious explanation, the fact remains that it operates in a kinship nexus and with reference to ideas, beliefs, and values about consanguinity. The Geertzes themselves posit a "polar tension between kinship and community" and tell us that "it amounts to a struggle between the principle that the fundamental bond among men is coresidence, sociality, and the principle that the fundamental bond is sameness of natural kind, genus" (p 167) "Sameness of natural kind" may be recognized in cultural categories other than kinship, but surely to posit sameness of kind as a "fundamental bond" and as a structural "principle" is only to say, in language more abstract than may be necessary, that "kinship amity" is a Balinese value—and one they share with many other peoples (see the work by M. Fortes mentioned earlier).

The Geertzes are highly critical here and elsewhere (see also C. Geertz, *The Interpretation of Cultures* [New York: Basic Books, 1973]) of mentalistic or cognitive-psychological orientations to "meaning" and "culture." As they see it, "meaning" is best regarded as an attribute or component of cultural systems, and cultural systems are best regarded as attributes or components of social systems Culture, as C Geertz would have it, is an "acted public document," and "culture is public because meaning is" Unfortunately, this argument appears to lead the Geertzes, as it has led other advocates of the hermeneutic perspective, to the unwarranted conclusion that we must abandon the time-honored (though not unchallanged) distinctions between linguistic and nonlinguistic meaning and between definitive and nondefinitive or contingent meaning Only if these distinctions are rejected does it make any sense to pass over virtually without notice, and certainly without explication, the Balinese expression glossed as "kinsmen"; to define "kinship symbols" with spurious precision as "certain symbols whose most immediate role it is to give meaning to the relation-

ships of people domestically involved in one another's lives" (p 156); to treat "ideas" about "the perceived nature of the connection between parent and child" as mere examples of "kinship symbols"; and to make a puzzle of the question "What is a dadia? Is it lineage? A caste? A cult? A faction?" (p. 157).

But there is no necessary incompatibility between recognizing the public, intersubjective nature of meaning and culture and recognizing also that what is structurally central to and culturally definitive of the Balinese "kinship idiom" and Balinese "kinship symbols" is the notion of relationship by genealogical connection. To recognize this is not to reduce "kinship" in Bali to an element of anyone's, or even everyone's, "cognitive map" (cf. p. 155), nor is it to suggest that the syndrome of social relations among kin should or can be studied as an "autonomous system." But it is to suggest that, even though there may be "no privileged zone in culture from which the others necessarily derive" (p. 157), there is, in the Balinese "kinship idiom," *a* central concept in the absence of which it would make no sense to call this a "kinship idiom" to begin with

Kinship in Bali is not convincing as an attempt to radically reorient anthropological thinking about kinship and kinship systems, and it is highly selective in its treatment of Balinese ideas, beliefs, and values relevant to their behavior as kinsmen. It is more successful, however--indeed quite admirable--as a concise and coherent overview of the structure of Balinese society. That in itself is no mean achievement and is sure to make this the standard reference on Balinese social organization.

Toward an Anthropology of Women. Edited by Rayna R Reiter New York and London: Monthly Review Press, 1975. Pp 416. $15.00.

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The scope of this book—sex role from gibbons to socialism—reflects the scope, both marvelous and maddening, of anthropology. The arrangement (from gibbons to socialism) also signals at least one strand of theoretical orientation of the book, a concern with social evolution. The anthropological study of women in itself invites an evolutionary perspective—the temptation to ask when (and how and why) the asymmetry of the sexes began is almost irresistible. But here in addition most of the authors are operating with a loosely Marxian orientation, which in contemporary anthropology can mean evolutionism, economic neofunctionalism, or simply a generalized critical stance.

One aspect of Marxian evolutionism involves the search for "the primitive," often construed as a mirror image, a simple structural inversion, of ourselves: where we are hierarchical, "they" are egalitarian; where we are patriarchal, perhaps "they" were matriarchal (at least once upon a time); where we are alienated, "they" are at one with their work, their social

relations, and their environment Paula Webster's paper in this volume, "Matriarchy: A Vision of Power," makes the extremely important point that the quest for such idealized forms is not a problem of "the facts" (which in the case of prehistoric matriarchies are so fragmentary that anything can be projected upon them, and nothing conclusive can be deduced) but a problem in the sociology of knowledge: the quest for matriarchies is both a reflection of historical and theoretical biases and an attempt imaginatively to construct a model of a nonpatriarchal world

As for primitive egalitarianism, Patricia Draper's paper on 'Kung Bushmen women tries to make a case for virtual equality of the sexes in 'Kung society. The paper is highly selective with its data, however, and Draper herself indicates that she has not dealt with the following domains of social life in which egalitarianism may or may not prevail: decision making, leadership, authority, marital relations, "access to extra-marital relations," and "the influence of young women in determining the selection of their first husbands" (pp. 94–95). Although the Bushmen clearly constitute a more egalitarian society than feudal Europe or advanced industrial North America, the case for their sex-egalitarianism has yet to be unequivocally made.

The paper by Ruby Rohrlich-Leavitt, Barbara Sykes, and Elizabeth Weatherford on Australian aboriginal women raises a different point in the problem of the search for the primitive Most accounts of Australian aboriginal societies show fairly clear male dominance in most domains of life—politics, marriage, myth, ritual. Leavitt, Sykes, and Weatherford want to argue that this view of these societies is skewed and biased by the "androcentrism" of Western research and that from a different perspective it is possible to see many sources of power and self-respect for aboriginal women. The basic point is certainly well taken (in the introduction Rayna Reiter has a good discussion of the special biases inherent in the ethnographic study of women), but it can be carried too far, winding up with an account that reinterprets every indicator of oppression, exclusion, devaluation, etc , as a projection of the ethnographer, while at the same time fastening upon every indicator of self-respect among the women as demonstrating absence of oppression The results—which give primacy to the native's point of view (the "emic" perspective) over attempts to apply external analytic or critical categories (the "etic" perspective)—are ironic for a Marxian approach. I doubt too that such denial of oppression would be made in situations of racism and or colonialism, and I find curious this tendency to try to analyze it away in the case of women.

What is at issue here, among other things, is the thorny question of women's "false consciousness"; the study of women, in my view, raises this problem more acutely than the study of any oppressed group Only the admirable paper by Susan Harding, "Women and Words in a Spanish Village," confronts the question directly. And as Harding says of the Spanish women's verbal skills, which seem to give them a great deal of informal power, "Each skill has a double edge, so that as it cuts for a woman, it cuts against her" (p. 307). That is, "The powers of village

women are very real, but within the given structures and relationships they do not constitute the last word" (p. 308). I do not particularly want to promote the phrase "false consciousness" (Harding does not use it, and I find it very misleading), but it is clear that women's own views of themselves and their society must always be understood in the context of an analysis of their total situation; most often (but not always) they operate within the terms of discourse set by a dominant culture that explicitly or implicitly devalues them. It is important that we begin to understand this disturbing process.

The final paper on primitive society that I will be able to discuss here (Harding's paper on a peasant society was introduced in relation to the false-consciousness problem) is Kathleen Gough's sober and useful piece, "The Origin of the Family." Gough asserts that "even in hunting societies it seems that women are always in some sense the 'second sex' " (p. 69), although the degree of subordination is variable from case to case. But she then goes on to try to differentiate such asymmetry from the real domination or exploitation that comes with the rise of the state. This I think is a promising line to take. Most people now working in this field would probably agree that things changed for women with the rise of the state, generally for the worse. It seems unlikely that such changes did not have some basis in prior structures and conditions. But Gough argues that the inequality in primitive societies "was based chiefly on the unalterable fact of long child care combined with the exigencies of primitive technology . . it was largely a matter of survival rather than of man-made cultural impositions" (p. 74).

What this suggests is the importance of distinguishing between different *forms* of asymmetry and developing a richer vocabulary of terms and concepts for apprehending our subject matter. As Reiter rightly says in the introduction, "We do not even know what we mean when we define a group as having 'male dominance' " (p. 15). What we need more than anything right now is a great deal of refinement of categories, growing out of confrontations with real cases. Weber's distinction between power and authority is one useful place to begin (see Ellen Lewin, Jane F. Collier, Michelle Z Rosaldo, and Janet S. Fjellman, "Power Strategies and Sex Role" [Paper presented at the 70th Annual Meeting of the American Anthropological Association, New York, 1971]). Another might be a distinction between relatively unrationalized sex-role "asymmetry" versus forms of "domination" that also tend to be highly rationalized. Interestingly enough, not only many "primitive" societies but also some modern Western cultures might fall into the "relatively unrationalized" (but still asymmetrical) category.

Two other articles in the book must be commented upon. Norma Diamond's "Collectivization, Kinship, and the Status of Women in Rural China" is a very effective piece which takes a specific problem—the persistence of patriarchal attitudes and practices despite the ideological intentions of the socialist state--and offers a neat anthropological explanation. Diamond shows that the organization of production collectives in China

tends to be based on village or neighborhood units, which in turn still tend to coincide with traditional patrilineages, complete with functioning exogamy practices Thus collectivization has inadvertently strengthened these classic patriarchal structures, and there is (at least) this social base for the reproduction of patriarchal attitudes.

Finally, Gayle Rubin's article, "The Traffic in Women: Notes on the 'Political Economy' of Sex," is in my opinion the centerpiece of the book. It is an elegant theoretical discussion, cross-fertilizing and reciprocally illuminating Marx, Freud, and Lévi-Strauss. Rubin argues that Lévi-Strauss's notion of "the exchange of women" is "neither a definition of culture nor a system in and of itself . . [but] an acute, but condensed, apprehension of certain aspects of the social relations of sex and gender" (p. 176) Most basically, marriage systems require two distinct and heterosexually inclined genders, male and female, and systematically work to produce and reproduce them "Far from being an expression of natural differences, exclusive gender identity is the suppression of natural similarities . . It requires repression . . " (p 180) (Many readers are likely to challenge the assumption that human beings are not naturally predominantly heterosexual– given the necessity for species reproduction— but Rubin's thinking is in line with accepted views concerning the relative lack of specificity of innate biological programming in humans) In any case, the argument then goes "deeper into the labyrinth," that is, into the psychological implications of this initial cultural split of the species But there is no way adequately to summarize the discussion within the present space, and I will simply say that the article has already established itself as one of the basic theoretical writings in the anthropology of women

Toward an Anthropology of Women also contains articles on primates (Leibowitz), on rethinking the man-the-hunter theory of human evolution (Slocum); on defilement in New Guinea (Faithorn), on updating Engels (Sacks); on Iroquois women (J K Brown); on men and women in rural France (Reiter), on cultural variation in "crisis" stages of women's lives (Silverman); and on aspects of the relationship between sex role and development/underdevelopment in the Dominican Republic (S Brown), Colombia (Rubbo), and Nigeria (Remy) There is also a useful bibliography.

Approaches to the Study of Social Structure. Edited by Peter M Blau. New York: Free Press, 1975. Pp ix+294. $12.95.

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This is a book of condensations of the recent theoretical work of Merton, Homans, Coleman, Parsons, Lenski, Bottomore, Lipset, and Coser, with various commentaries. For the most part, these people have written more extensively elsewhere on the same ideas, so the main function of the work

is to provide convenient summaries for people not enough interested in social structure to read the originals It seems to me I ought not review the book, except perhaps to advise the authors that if they have nothing new to say, they need not say it.

Let me, however, quote one passage from Parsons:

> The highest [internally equal status and prestige grade in Universities] we conceive to be senior faculty status, which has traditionally come to carry full tenure, we have however, extended the concept of tenure to include not merely the senior faculty but also junior time-limited faculty membership and membership in student capacities, both graduate and undergraduate At any given level of tenure we conceive members of the same university to be equals of each other, but the system as a whole is stratified on the basis of levels of commitment to and competence in the implementation of the value of cognitive rationality Thus it can hardly be conceived that the average undergraduate freshman is equal in these two respects to the career-committed senior professor [Pp 107–8]

It is hard to know what to say about such a passage, but I hope it brings comfort to CCNY assistant professors and makes them realize that the reason they have one-week tenure toward the end of each month is that they are not very committed to and competent in the implementation of the value of cognitive rationality. Perhaps the retired professor's lesser tenure rights are due to the same cause.

The interesting part of the book is Walter Wallace's commentary on Parsons and Coleman, so I will devote the rest of this review to those 14 pages. What Coleman and Parsons have in common in these two papers is that they address the question of how rewards and punishments for some action come to be lodged in particular social locations That is, we ordinarily reward people not with gratifications from our own bodies or our own gardens, but with money, political decisions, grades in school, etc., whose value derives from the social order. The "psychological reductionism" of both Parsons and Coleman is eminently sociological, in the sense that the rewards each person or role has to pass out depend on the social validity of the chits he or she holds, not on the inherent gratification of getting a dollar or a vote. "Put your money where your mouth is" is the folk counterpart of this fundamental observation that we "really mean" something only when we are willing to commit our socially valid chits to it.

Coleman's and Parsons's divergent treatments of this problem, which Wallace analyzes so subtly, allocate causal priority in slightly different ways, comparable to the difference between macroeconomics and microeconomics. And just as it cannot be, in the long run, that the generalizations about deficit spending and those about consumer rationality contradict each other, so the truths, in Parsons's and Coleman's essays cannot contradict each other.

What Parsons wants to know is how the requirements of the achievement of a certain value in one sector (say a university) that require a performance in another sector (say NSF) get transmitted in an authoritative way. The general notion is that if one is going to have training and

research located elsewhere than is political budget-making, the society "needs" regularized ways for NSF to tell people whether they can go ahead, and we university types "need" to be able to tell the political system that otherwise the Russians will get ahead of us. Parsons's argument is that we do not really convince Senator Proxmire that genetic research or satellite reconnaissance is good for the competition of the Maryland crab business with Black Sea crabs, but rather that we exchange general faith in the university or in the republic of science (as concretely manifested in the exchange and review of research proposals) for general adaptability to national needs.

The "media" with which we communicate, then, have to transmit carefully judged information about "needs" of a subsystem, such as university research, and these media have to be, in some sense, "authoritative" in the other system, such as NSF Money is the ideal-typical "medium" because it can be accounted for in detail in each system, can be transmitted between systems, and is authoritative in most interpersonal or interorganizational transactions Parsons is interested in the fact that all sorts of promises in exchanges get their social validity from the flows of such media and that the problem of CCNY is not that they do not want to give tenure, but that they do not have enough money If they are to get enough money it must be by some authoritative "medium," through which they can communicate to other social units, that they have to pony up

Coleman is interested in a different aspect of the same problem Suppose that one's control over media (votes, money, recognition through grades, etc.) is set and what one wants to achieve (either as a person or as an incumbent in a role) is set Then what will be the resulting pattern of social transactions? Wallace has clearly seen that Coleman, like the economists, postulates a purposive man, with preferences given, in order not to worry about psychology and in order to go instead directly to the structure of exchanges That does not mean, as Wallace almost alleges, that Coleman thinks preferences are in fact determined outside the social system, by genetic or physiological mechanisms. It just means that even Coleman can't think about everything at once His question is how a social system with such exogenous determination of preferences and powers can work. What will be the collective decisions, what the distribution of power over outcomes, what the degree to which each person's exogenously determined interests will be satisfied, and what the relation between the person's actions and those preferences, as behavior is shaped by the exchange situation the person is in?

Wallace is little inclined to vain debates about whether one theory overemphasizes rationality and the other overemphasizes values The question Parsons asks, while casually extending tenure (but not too much tenure) to freshmen, is how causes are transmitted from one social formation to another in a socially organized way They must be transmitted from one to another if a differentiated structure is to serve the same set of functions as the undifferentiated one did, for the functions must be adapted to one another. Consequently an evolutionist must be interested in media. The

question Coleman asks is how arenas like legislatures or trade unions work, when specific persons bring in their socially validated media of exchange (votes, money, etc.) to try to cash them for what they want. While it is the effectiveness of the media in those arenas that entrances Parsons, the effects of those media clearly work themselves out through the mechanisms Coleman outlines.

The upshot of Wallace's analysis is, then, that Coleman and Parsons are analyzing different aspects of the same thing, that Parsons's abstract concepts are made flesh in Coleman's mechanisms, and that Coleman's mechanisms have their larger significance in social structure for the reasons that Parsons gives Wallace does not quite say that, but his discerning analysis of what Parsons means by media (or rather, what Parsons must mean as inferred from the role they play in his argument, since Parsons never says) and of what Coleman's radical assumptions about rational men pursuing predetermined interests really do for his overall intellectual structure brings those two sociologists' works much closer to each other.

My own faith is that valuable thoughts about social reality cannot really contradict each other. By setting himself against utilitarians, Parsons early on committed himself not to listen to people like Coleman By doing his own work instead of also doing Parsons's, Coleman has partially sacrificed seeing the structural implications of what he takes as given for the purposes of analysis So by being uncommitted on the question of utilitarianism, and also interested in the question of how things that are given for the purposes of analysis come to be given, Wallace has uncovered a deep convergence between Parsons as the leading antiutilitarian and Coleman as the leading utilitarian

Whether these 14 pages justify publishing a book I leave to the new economics They justify some new deep thought.

Internal Colonialism: The Celtic Fringe in British National Development, 1536–1966 By Michael Hechter. Berkeley: University of California Press, 1975. Pp. xviii+361. $15.75

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Although ostensibly a history of the role of the Celtic periphery in the political development of the United Kingdom, this book is actually an important work in general sociology touching on such central theoretical problems as the growth and integration of nation-states, the spread of imperialism and underdevelopment, the assimilation or segregation of ethnic minorities, and the origin of ethnicity itself. It also represents an important advance in the application of sophisticated quantitative methods to the analysis of historical data, and although the quality of such data necessarily declines as a function of temporal distance from the present, Hechter's quantitative analyses are ingenious, theoretically provocative,

and for the most part convincing. The book is full of complex, evocative argument and intricate subanalyses which, while advancing the general line of argument, provide insights into such diverse topics as the relationship between evangelical Protestants and the rise of industrialization, the determinants of imperialist voting patterns in the working class, and the causes of the political secession of territorial subunits.

The central theme is the relationship between political, economic, and social development and the assimilation of ethnic minorities in nation-states. It generally has been assumed that processes such as the expansion and growth of a territorial state bureaucracy, the spread of citizenship and literacy to the lower classes, the occupational differentiation and integration accompanying industrialization or the breakdown of primary group ties associated with urbanization will lead to the integration of ethnic minorities into the nation-state This assumption is based largely on observation of the new nations of the 1960s, particularly African nations, where patterns of ethnic cleavage, often inaccurately called "tribalism," seemed to contrast markedly with the putative ethnic solidarity and political stability of the industrialized West This perspective, which Hechter terms "diffusionism" and which he associates with the work of Parsons, Deutsch, Pye, Almond and Verba, and Lipset, is in fact supported by cross-sectional data since it can be demonstrated that indices of industrialization, urbanization, and political development are correlated with linguistic and ethnic homogeneity. Cross-sectional data, or for that matter any data at all, are frequently the best that can be hoped for in the study of national development, but observations at one point in time cannot provide a definitive test of a causal argument depending on processes occurring over a long period of time In the case of mother-tongue diversity, for example, Lieberson, Dalto, and Johnston have recently demonstrated (*AJS* 81 [July 1975]: 34–61) that cross-sectional data may be seriously misleading since their analysis of historical data from Western states indicates that national development has no appreciable effect on linguistic heterogeneity despite a strong positive cross-national correlation between development and homogeneity. Historical data such as those used by Hechter provide a rare opportunity in an observational science like sociology to examine causal sequence, and his method represents a powerful new tool in a discipline accustomed to analysis of survey data collected at a single point in time or at most over a few years or decades Hechter's data include measurements at 11 points in time for the period from 1851 to 1966 for 118 counties in England, Scotland, Ireland, and Wales, and his results cast considerable doubt on theories derived from cross-sectional data.

Hechter contrasts the views of the diffusionists with a perspective based on what he calls the theory of "internal colonialism" which evolved from studies of uneven development in Latin America and race relations in the United States. According to this view, once an ethnic group or region has been assigned a subordinate functional role in the national or international division of labor, ethnic identity is likely to persist or even intensify in

reaction to the ethnicity of the economically dominant occupations or sectors. Ethnic diversity decreases, according to this view, not through the diffusion of industrialization or national political culture throughout a society but rather through explicit national policies aimed at breaking up the ethnic division of labor and eliminating regional economic differences. In this view there is little difference between a peripheral region within a nation-state which has limited access to the power or prestige of the dominant culture and an overseas colony whose culture and power are similarly devalued relative to those of the metropole

The United Kingdom would seem to be a hard case for the internal colonialism hypothesis. Despite Dr Johnson's dyspeptic view of the eating habits of Scots, the United Kingdom is usually presented as a model of national integration, political civility, and (Commonwealth immigrants excepted) ethnic tolerance persisting despite a defunct empire and a declining pound. The view from the Celtic periphery of Wales, Ireland, and Scotland is, Hechter argues, somewhat different. Each of these regions was incorporated into the United Kingdom after the consolidation of the Tudor bureaucracy in England—Wales by annexation in 1536, Ireland by Union in 1801 after a long series of military campaigns, and Scotland by economic blackmail in 1707. Each region had a distinct culture, language, and political structure at the time of incorporation, and each received only nominal rights within the United Kingdom. Despite the fact that England had successfully incorporated Frisians, Danes, Picts, and even invading Normans almost without a trace, Hechter demonstrates a surprising persistence of ethnic identification on the Celtic periphery and a persisting dualism in the British economy.

Drawing on British census, marriage, and voting records for 1851-1966, Hechter demonstrates the persistence of Celtic cultural characteristics either directly through the pattern of nonconformist religious affiliation or language use or indirectly through the relative size of the residual error in the prediction of voting patterns from development variables alone. In general he indicates that despite the expansion of British industry during this period Ireland, Scotland, and Wales were as far or farther behind economically at the end of the period as they had been at the beginning and that fluctuations in relative economic position are unrelated to the progress of industrialism Similarly, religious resistance to the Anglicization of the Celtic periphery persists and in Wales has even increased over this period despite industrialization, urbanization, and state expansion Only in the case of linguistic diversity does Hechter find a distinct tendency toward convergence with the dominant culture, but it is a result of conscious government educational policy aimed at eliminating Celtic languages. In Ireland, however, this trend was reversed after partition and Gaelic speaking increased in the republic. Increasing industrialization did provide an opportunity for political integration through the organization of industrial unionism and the integration of minority interests into the Labor party in Wales and Scotland but not, of course, in Ireland. The rise of the Scottish National Party and Plaid Cymru in Wales, however, indicates that even

this degree of political integration may rest on a fragile base and function best when Labor is out of power. Imperialism has not proved a successful instrument in national integration since critical elections revolving on international questions, such as the "Khaki" election over the Boer War in 1900, do not show an increase in peripheral support for the Conservatives. (On the contrary, Welsh voters were disproportionately opposed to the government's imperialist adventures.)

This brief summary does not give due credit to the subtlety of Hechter's argument, the ingenuity of his analysis, or the balance of his conclusions. He gives careful attention to numerous alternative explanations of his results and indicates those areas, notably language, in which the diffusion theory receives considerable support. Some readers, including me, will object to the use of the term "colonialism" to describe political differences within the United Kingdom which are trivial in comparison with those separating colonizer and colonized in preindependence Angola or with the legal and cultural differences separating *mestizo* and *indio* in Peru A more modest term such as peripheral sectionalism used in chapter 7 would have caught the concept as well, despite perhaps sacrificing some revolutionary élan. The book also suffers from some redundancy resulting from integrating previously published articles into a continuous narrative. These problems do not, however, detract from a work which combines important theoretical questions, relevant and well-analyzed data, and a dense network of suggestive hypotheses

Race, Class, and Power: Ideology and Revolutionary Change in Plural Societies. By Leo Kuper. Chicago· Aldine Publishing Co , 1975. Pp xiii+ 345. $14.75.

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Three main themes characterize these 10 essays, three of which are published here for the first time: (1) Against orthodox Marxists, Leo Kuper maintains that racial divisions in plural societies constitute "phenomena in their own right" with "structural significance" (p 269) and, therefore, are not reducible to economic conflicts alone. (2) Against the revolutionaries of all ideological shades with their "platitudes of violence" (p. 141) and shallow rhetoric, Kuper explores the possibilities of relatively peaceful, evolutionary change in seemingly polarized racial confrontations. (3) Against the widespread denigration of political liberals, Kuper reformulates liberal philosophy of interracialism in the face of its rejection by both antagonists in colonial-nationalist struggles of liberation. Data from Algeria, Rwanda, Zanzibar, occasionally the United States, but above all South Africa serve to substantiate and illustrate integrated comparative reasoning.

One could think of few more controversial, complex, and theoretically

important issues for a historically informed analysis of the contemporary political scene in ethnically heterogeneous Africa. A former South African sociologist with additional legal training and political commitment, the author tackles the topics with an undogmatic sophistication in the best tradition of sociological scholarship Here is a rich source of structural analysis which relentlessly probes beyond the fashionable assertions of true believers and conventional wisdom. Yet many questions remain, and one is left unconvinced on some crucial points.

Only orthodox Marxists will not admit the failure of intersectional working-class solidarity in ethnic conflicts. Ever since the German Social Democrats voted for the imperial war budget at the outbreak of the First World War in 1914, this socialist dream received continuous blows until its present spectacular defeats in Northern Ireland, Lebanon, Cyprus, and Southern Africa In culturally plural societies, people are more easily mobilized on the basis of "primordial identities" than on that of cross-cutting common interests which remain abstract. Kuper, along with Marxists, explains this irrational appeal with rational reasons of benefits of power for the contenders of scarce resources, be it a higher price of labor for one's group members or other privileges of dominance. This focus, however, neglects the psychosocial background of a culture-specific common socialization experience. At a fundamental level, in times of anxiety and conflict, this bond offers relief, because it is associated with the comfort and security of early childhood, while the socialist unity with the stranger on the other side remains a doubtful promise. Herein lies the secret of the revival of ethnicity everywhere. In a review of the Simons's application of the theory of class struggle to South Africa, Kuper superbly demonstrates the flaws and contradictions of the Marxist approach. Undoubtedly, the perception of groups in conflict is not merely an epiphenomenon of a superstructure but as significant a factor in revolutionary upheavals as are the structural relationships of "differential incorporation." Many influences have shaped the ideologies of colonizer and colonized, and Kuper justifiably views them as a seismograph of race relations.

Despite polarization and increasing rigidity in the racial structure of societies dominated by white settlers, Kuper draws attention to the so-called continuities of interracial association and interdependence. He constantly wavers as to which of the opposing trends should be given more weight and commits himself only to "potentials." Relatively abstract theorizing about economic interdependence, for example, cannot lead to firmer conclusions, but a concrete analysis of strike situations, changing labor requirements, and investment figures probably could. In this respect, focus on the political economy of South Africa in its global interdependence by undogmatic "Marxists" would seem to render more insights into likely future developments, than Kuper's somewhat vague consideration of economic factors among others. Indeed, with third party involvement in Southern Africa, the level of conflict and violence will depend largely on outside developments. Kuper convincingly maps out a wide range of possibilities, including concessions by a pragmatic oligarchy and political initiatives

by South African blacks through apartheid itself. All of this makes the frequent predictions of inevitable revolutionary violence, let alone its success, highly dubious.

However, Kuper as a liberal also expresses strong moral repugnance about such Fanon-inspired "dehumanizing concepts as that many peoples are destined for the rubbish-bins of history, or that power grows out of the barrel of a gun" (p. 272). In contrast to the "reasoned interracial appeals" (p. 274), he has no illusions about the widespread persuasiveness of the call to arms. Faint is the hope that this fatal cycle of liberation through heightened repression will ever be broken To the moral aspect one could add the pragmatic consideration that the violence of the oppressed may often make conditions worse for everybody, themselves included But who can decide about this cost calculation other than those who want to take the risks on both sides? Instead of cheering the antagonists from their academic shelters and pulpits, the only task sociologists can justifiably perform lies in enlightening the activists about the implications of their confrontation stance as well as the many avenues of potential compromise available In this task, Kuper's analysis has set an impressive example

Dependence and Transformation: The Economics of the Transition to Socialism. By Clive Y. Thomas New York and London Monthly Review Press, 1974. Pp. 327. $10.95.

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This is a significant work in two important respects It is the first explicit Marxist approach to underdevelopment and dependence to come out of that group of Caribbean economists calling itself the "dependency school" At the same time, it is an original contribution to the theory of the transition of small economies from underdevelopment to socialism.

Thomas rejects both the neoclassical and the traditional Marxist pessimism about the effects of size on the potential for transformation, maintaining that size is the context rather than the absolute determinant of structural dependence and underdevelopment He argues further against the assumption that size can explain product and market specialization of peripheral underdeveloped economies, without taking into account the history of imperial-colonial domination that accompanied the international expansion of capitalism.

Such expansion created a "neocolonial mode of production" with cumulative divergences in resource ownership, themselves complemented by various forms of structural, psychological, and cultural dependence. These divergences are specifically reflected in the high level of foreign decision making and institutionalized leakage of local surpluses to owners of capital, technology, and managerial skills in the metropole.

Structural dependence, as reflected in the heavy reliance on foreign

capital, technology, and export-market instability, assumes a qualitatively unique character in small economies, where a reciprocal relation between dependence and specialization evolves. First, global specialization generates such a high degree of dependence (and integration) of small economies on international capitalism that this dependence becomes "functionally autonomous," and structural dependence in turn results in a high degree of export specialization. Second, the institutional forms of the divergences in small economies are marked by a totality of dominance and control (e g., the slave plantation, the multinational corporation).

Solutions to these problems are presented in part 2. It is proposed that if small economies are to be more than appendages on the periphery of international capitalism, they must be transformed from dependence to socialism. This process of severing the links with international capitalism is to be accomplished through state ownership of the dominant means of production, decisive state intervention in external relations, and various strategies of converging the divergent patterns of resource use, production, technology, demand, and needs. The basic contradiction between production and distribution and the urban-rural and intrarural class contradictions are also to be addressed The formation of a worker-peasant alliance and the adoption ot the principles of social and collective consumption and democratic participation at all levels of the process of production and distribution are also proposed

While small economies such as exist in the Caribbean form one of the referents for Thomas's analysis and recommendations, there are some basic questions about the analytical usefulness of his concept of the "neocolonial mode of production" and the descriptive adequacy of its application to the structure of resource ownership in the Caribbean The concept of neocolonialism, first popularized in the early 1950s, suggests various forms of external control and economic dependence after the formal recognition of national sovereignty. Similarly, Thomas's concept of the "neocolonial mode of production" rests essentially on the dominance of foreign ownership of the means of production and external dependence.

These two factors alone, which were from the beginning the basic features of Caribbean political economy, do not, however, adequately specify its mode of production during the colonial period, nor do they now define a unique mode of production (of recently changing institutional forms) assumed to have evolved after "flag independence," though they aptly describe the long-term character of Caribbean socioeconomic formations. If the dominant mode of production in the Caribbean or other neocolonies was not significantly altered when the granting of constitutional independence redefined these units from colonial to neocolonial status, not much purpose can be served by the loose application of the concept of "neocolonial mode of production." It would be necessary to demonstrate whether the changing character of Caribbean economies represents a distinct mode of production or merely a different phase in the same production regime.

In actual historical terms, a full description of the dominant mode of

production, especially in the Commonwealth Caribbean, must take into account, besides factors of external ownership and dependence, the unique character of the divergence of local ownership with which such external ownership has been conjoined, as well as the historical character of the organization of labor. What has remained striking about the structure of resource ownership and the organization of labor in many Caribbean economies is the significant role the factors of race and ethnicity have played in this divergence Not only class but also race ownership of the means of production has been the general rule While the domestic racial minorities only partly owned the means of production, they were full dominants in the legislature, the militia, the church, the schools, and the courts over long periods of colonial history and themselves served to perpetuate colonial domination

The assault on the plantation system via collective labor movements, the granting of universal suffrage, and constitutional independence had the effect in some parts of the Caribbean of differentiating the economy and polity along racial and ethnic lines At the same time it is increasingly obvious that black governments in the Caribbean, with the same colonial domestic and external relations, have intendedly and unintendedly co-opted the demands of the working class for full equality and that the "powers behind the throne" are still the international ruling class, the traditional minorities, and the comprador bourgeoisie which is itself integrated into the structure of metropolitan domination Yet the significant role of race in the divergence of ownership and control of domestic resources and political change in the Caribbean cannot be ignored without a distortion of Caribbean history, just as its importance in the international system cannot be denied An adequate theory of dependence and underdevelopment of the Caribbean would take account of the character of domestic resource ownership and the fact that production relations continue to be characterized by a racial-ethnic division of labor.

There is hardly a reference to this problem in Thomas's formulation In ignoring the factor of race, he seems to reject his own recommendation that race should be integrated as an essential feature of Marxist theory This criticism of his approach is not to deny, however, that the conditions for the meaningful solution of the race problem are created by a socialist transformation. Thomas has clearly defined the strategies necessary for this task.

Sociological Theory and Survey Research: Institutional Change and Social Policy in Great Britain Edited by Timothy Leggatt London: Sage Publications, 1974 Pp. ix+334. £6

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In his introduction to this volume Morris Janowitz states, "The intellectual history of British sociology reflects the dominance accorded to the

social survey tradition rather than to the continental approach by way of grand theory" (p. 9) This is not strictly true. The survey tradition has been strong in Britain but largely outside the mainstream of academic sociology. The pioneers of the British survey tradition, Charles Booth and Seebohm Rowntree, were businessmen and had no academic affiliation. The New Survey of London and the Survey of Merseyside were carried out, in the 1930s, under academic auspices but were directed by researchers in public and social administration. Subsequently, Louis Moss founded the Government Social Survey which has undertaken many studies with explicit policy orientations Various market research organizations have conducted surveys of political and social behavior that have been subjected to secondary analysis by a few academic sociologists. But the majority of the latter have remained largely hostile to social surveys. There is no university-based survey research center in Britain, and the Survey Unit of the SSRC, under whose auspices the papers in this volume were prepared, was intended primarily as an advisory body and has since been disbanded. It is no coincidence that only three of the 12 contributors to the present volume are sociologists holding academic appointments at British universities.

Survey research in Britain, as in other countries, has suffered from its relative isolation from sociological theories, even those of the "middle range" It has tended to be empirical in the worst sense of that term, reporting quantitative findings with little attention to conceptual frameworks or explanatory models. The essays in this volume arose out of a seminar which aimed to stimulate the application of sociological theory to survey research and to examine the specific contributions that the latter might make to the former. The volume is worth reading if only as a salutary reminder that there is still a wide gap between those who specialize in the generation of theories of society, critical or otherwise, and those who attempt to verify these theories with evidence from survey research.

The research reported ranges from a study of family size and spacing using a sample of married women in one city to studies of apprenticeship, white-collar unionism, and management style in various industries Also included are two studies dealing with voting behavior and political participation, one with the social construction of local communities and another with the correlates of prejudice. These are based on samples designed to be representative of the country as a whole, or of its major urban areas Details of sample design and selection are not provided in all cases but appear to have been carried out scientifically This cannot be said of the questionnaires or interview schedules used. Several of the reports are based on a secondary analysis of data collected for other purposes, but this is not sufficient to explain the apparent lack of concern with problems of validity and reliability in scale construction. Several of the studies rely on single-item responses for dependent and/or predictor variables, and those which combine items into indexes report no information on their reliability. They appear to ignore the question of measurement error. This is particularly

serious when techniques such as multiple correlation, regression, and path analysis are adopted. As is common in survey research, the proportion of variance in the dependent variable explained by any of the studies reported in this volume is lamentably small Whether this is due to measurement error, poor theoretical constructs, and/or inadequate conceptualization of the problem, or whether it is inherent in complex situations where heterogeneous populations are being studied, is a question methodologists have yet to answer satisfactorily

A study which exhibits all the weaknesses of research based on the secondary analysis of survey data is R. T. Schaefer's "Correlates of Prejudice" The author reanalyzes data originally collected in connection with the Survey of Race Relations in Britain in 1966–67. He appears to be unaware that these data had already been reanalyzed by Christopher Bagley, who demonstrated the unreliability of the original prejudice scale and the relatively small proportion of variance that could be explained in terms of demographic and structural variables (*Social Structure and Prejudice and Five English Boroughs* [London: Institute of Race Relations, 1970]). Nevertheless, after explaining only 11.5% of the variance in his measure of prejudice, Schaefer claims support for the hypothesis that "a person's racial attitudes are strongly conditioned by his position in the social structure and are also deeply influenced by a situational component reflecting the extent and nature of his interaction with minority group members" (p. 237). The original study contains items that were designed to measure authoritarianism and other personality variables, but no attempt was made to assess the additional contribution these may have made to explaining the phenomenon of prejudice.

As a practitioner of survey research and a student of British institutions, I learned a lot from reading this book, but I am sorry to admit that it will give ammunition to those who reject social surveys as a methodological tool in sociological research.

The Sanctity of Social Life: Physicians' Treatment of Critically Ill Patients By Diana Crane New York Russell Sage Foundation, 1975. Pp. xvi+285 $13.50

Bradford H Gray

National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research

Who can be unaware today of developments in medical research and technology which have given the medical profession new control over life and death? These developments have raised a number of fascinating and perplexing legal, ethical, and social issues, among them the criteria used in decisions to apply the new technologies at both ends of the life cycle. Norms heretofore in harmony—for example, those supporting the alleviation of suffering and those upholding the sacredness of life—are brought

into conflict. Existing wisdom, traditional medical ethics, and the law have all been unprepared for the new circumstances which are continually being presented. Not surprisingly, these developments have been accompanied by growing concern over the physician's autonomy, concern reflected in new review and monitoring procedures and an expanding notion of informed consent.

Diana Crane's book, *The Sanctity of Social Life,* is an important empirical contribution to the rapidly growing, multidisciplinary literature that has developed around these and related issues. Using a variety of methods —survey research, observation, and the study of records —Crane provides us with data on variations in physicians' approaches to situations where life and the timing of death are optional

Much of the book is based on the responses of physicians from various specialties to some realistic case examples designed to elucidate the types of nonmedical criteria which affect the physician's response to pathology. These case examples, which present situations ranging from treatment of damaged newborns to resuscitation of patients who have died of terminal disease, were developed with obvious care and represent in themselves an impressive accomplishment. (They are contained in the book's appendix.) They allow us to begin to understand the effect on physicians' decisions to treat, resuscitate, or withdraw treatment of such variables as the patient's "salvageability," the presence of physical or mental damage, patient and family desires, age, deviant statuses, financial factors, and stigmatizing conditions. Since appropriate case examples were developed separately for each specialty group sampled—neurosurgeons, internists, pediatricians, and pediatric heart surgeons—and since each case was developed to test the effect of particular variables, economical presentation of the data must have been a challenge. However, the author presents the results of her analysis clearly and succinctly and provides valuable summaries throughout.

Crane has provided us with a wealth of evidence concerning the ways social criteria affect medical decision making She finds, for example, that physicians' decisions regarding intervention are consistently influenced by the likely effect of physical or mental damage suffered by the patient on the patient's ability to perform social roles. Her evidence, though not unequivocal on this point, suggests that the physician is influenced more by patients' capacity to perform social roles than by the social value of the roles. While family attitudes are influential, particularly when the patient is a child, there are some interesting differences between the doctor's perspective and that of the layman. In particular, the physician makes a greater distinction than the layman between patients who can be maintained over a considerable period of time and those whose condition is clearly terminal. Thus, this area, Crane predicts, will be one in which serious conflicts between physicians and patients or their families will arise, a situation which has subsequently gained familiarity in the Quinlan case.

Crane does not rely solely on survey evidence, however. Certain of her case examples lend themselves to independent validation through examina-

tion of hospital records For example, the frequency of resuscitation in the presence of certain conditions can indicate the extent to which the actual behavior reflected in the records matches responses to hypothetical situations. Although there are some discrepancies (e g , in answering the questionnaire physicians seem to have overestimated their willingness to perform heroic measures), the data are largely reassuring with respect to the validity of her survey results.

Crane then turns to an analysis of the sources of variations in physicians' decisions, focusing particularly on the type of medical school, the prestige of the hospital, colleague consensus, and departmental policies Her description of the impact of the particular concerns of residents, compared with those of physicians who have completed their training, is most interesting Crane suggests that the residents' greater orientation toward active intervention (e g , regarding resuscitation) is related to their desire to obtain experience. The effects of religion are also examined Overall, liberal Protestants are found to be less concerned with the preservation of life than are Catholics or Jews, but Crane finds this to be more a matter of social status than of religiosity. Overall, Crane concludes that activism in the preservation of life is not, for the most part, a matter of organizational variables but rather of deep-seated cultural and religious attitudes concerning the value of life

A chapter is also devoted to Crane's observations and interviews with physicians and medical personnel on two cancer chemotherapy wards. Her focus was on control and decision making regarding treatment of patients. She adds further support to the concerns raised in earlier studies about informed consent and the possible contamination of patient-care decisions by research considerations.

In summary, *The Sanctity of Social Life* is a valuable addition to the literature on a fascinating set of problems Crane shows the extent to which physicians are using a "social" definition of life and are thus using social criteria in decision making rather than following "traditional" norms supporting the use only of physiological criteria While Crane's analysis is firmly tied into the sociological literature- her analysis of the effect of religion is a prime example—she is also sensitive to the policy issues which have made these matters of interest to courts and legislatures as well as to the medical profession and its analysts and critics

Ethical and Legal Issues of Social Experimentation. Edited by Alice M. Rivlin and P. Michael Timpane Washington, D.C.: Brookings Institution, 1975. Pp. xi+188. $9 95 (cloth); $3 50 (paper).

Sissela Bok

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In an effort to avoid the haphazard, intuitive policy changes of the past, governments have recently increased their reliance on social experimenta-

tion. A new tax incentive plan, a changed curriculum in high schools, or a new traffic pattern may be tested on a sample of the population so as to evaluate and perhaps redesign the policies before they are initiated on a large scale. The effects are often negligible even if not benign for any one individual participating in such studies Yet at times human interests are powerfully affected, individually and collectively, in ways that call in question the propriety of the experiments and the adequacy of the safeguards used.

Not only individuals, but entire ethnic or social groups are now becoming more aware of the stigma which can result from experimental findings. They are more concerned about being manipulated. And their confidence in governmental benevolence and accountability is at an all-time low.

It is urgent, therefore, to begin an informed, broad-gauged debate to consider what individuals or groups stand to gain or lose from different degrees of regulation of these studies. We need to sort out those studies which carry no risk from those which do, those which may bring valuable knowledge from those so poorly designed that nothing can be learned from them; and those which bring new knowledge from those which merely repeat earlier studies, even ones already discarded elsewhere.

Ethical and Legal Issues of Social Experimentation provides an admirable basis for such a public debate The volume offers a wealth of empirical information and an understanding of the diverse interests affected, in a lucid and well-organized presentation of views. The papers, originally presented at a conference at the Brookings Institution, not only function as articulate expositions of the problems from different professional points of view; they serve also as examples of the very different attitudes one can adopt toward practices of social experimentation

The first chapter, by Alice M Rivlin and P. Michael Timpane, introduces the legal and ethical dimensions of social experimentation. The authors discuss issues of fairness, of confidentiality, of compensation, and of consent, as well as the larger question of whether some experiments, no matter how protective of these dimensions for human subjects, should be ruled out as immoral in their own right. They end with a thorough checklist of questions that "prudent investigators might want to ask themselves when planning a social policy experiment."

Chapters by Robert M. Veatch and Peter G. Brown give clear and detailed attention to the experience of the biomedical sciences with guidelines and to ways in which social experimentation can and cannot be compared with biomedical experimentation. They present views on informed consent which, along with the comments by David N. Kershaw and Joseph P Newhouse and the chapter by Edward M Gramlich and Larry L Orr, shed light on the considerable differences with respect to the nature of requirements for consent in social experimentation. Gramlich and Orr describe in detail an ongoing health insurance experiment in order to demonstrate how they think consent and subject protections should operate in such a context

Charles L. Schultze compares social experimentation with social policy in general from the point of view of the harms and benefits which can

flow from them and the rules which guide them. He admits that his description of the rules safeguarding human rights in social programs is "relatively idealized." The prevalence of such a stance among those who design and evaluate programs and experiments bespeaks the need to hear also from those most affected by the various schemes. They are not directly represented in this volume.

Alexander Morgan Capron considers from a legal perspective the separate and, at times, conflicting interests of the various participants in social experiments He gives readers as lucid and distilled a view of the legal roots of the concepts of "privacy" and "informed consent" as they are likely to find anywhere in the literature Social experimenters have much to learn from the long experience of the law with these issues Psychologists, anthropologists, and sociologists have as yet had less opportunity than those doing biomedical research to consider these issues as they affect professional responsibilities and have been much less strictly regulated Economists, who perform or supervise much social experimentation, do not even have a professional code of ethics to guide them in the first place

It is, nevertheless, an economist, Thomas Schelling, who presents, in the final chapter, the kind of incisive, thoughtful analysis which should guide any attempt to set ethical and legal standards for social experimentation. He provides a "typology of social experiments" which classifies them according to a number of criteria In addition to furnishing crude measures of harm and benefit, he suggests that we ask whether subjects can screen themselves so as to opt out of experiments, whether results might ever be used *against* the subjects, what other uses might be made of the results and *who* is going to make use of them, the degree to which subjects can be identified before and after an experiment, the degree of urgency of the research, and the nature of the policy alternatives available Other criteria can be added to this list: the degree to which the study is likely to produce valid information in the first place —something which cannot be taken for granted in our overresearched society, where too few experiments are adequately constructed so as to be capable of yielding valid data—or the likelihood that the information cannot be acquired in other, nonexperimental ways, or ways using retrospective, rather than prospective, techniques.

A typology such as that suggested by Schelling can be worked out in detail only with help from a number of disciplines And it ought to be restricted neither to *social* experimentation as opposed to other kinds nor to *experimentation* as opposed to more traditional ways of affecting human welfare. It may seem paradoxical that so much attention has been focused in the past decade on experimentation rather than on all the less systematic interventions in the human condition This is true in medicine as well as in psychotherapy and government policies A study, so designed as to produce results, can be scrutinized in ways that more casual, less documented therapy or policy can escape. Yet both of the latter have clearly brought much more human misery through the ages than have experimental studies. The answer, however, is not to abandon the concern for human needs and rights in research; we must, rather, consider more carefully the

moral dimensions of *all* professional or governmental interventions in human lives, whether of a research nature or not, and the forms of accountability and incentive structures which can lessen abuse.

The analysis provided by this book can help to stimulate a debate concerning these matters. We must hope that such a debate will both contribute to stemming the tide of overlapping and sometimes conflicting regulations and provide stronger protection for the vulnerable.

Comparative Community Politics. Edited by Terry Nichols Clark New York: Sage/Halsted/Wiley, 1974 Pp. 415. $17 50.

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Who does community power studies in the 1970s? According to the selection of articles in this book the answer appears to be "those who want to make a trip across the Atlantic " Brought to a sudden halt in the late 1960s by the work of John Walton and others, who showed that initial assumptions and methods determined what was discovered, enthusiasts in the search for community power paused for new inspiration. There had to be *some* return on all that intellectual investment in "decisions" and "power" in "communities." The idea that the old material could be used all over again by comparing it with material gathered somewhere else had great charm. The lack of a methodology for comparative analysis was no deterrent. When so little is known about far away places, it was argued, there may be no harm in starting with the stories

However, stories without "theory" do not get one very far in the bustling bazaars of modern sociology. Terry Clark is concerned in this volume with advancing the systematic study of comparative community politics and, to this end, in his introductory chapter he advances 48 propositions for consideration. Some of them seem hard to refute, and some are surely tautological. Clark makes much of the point that the more things are decided centrally the less can be decided locally or, conversely, the more resources local communities have the more things they can do His approach puzzles me, since the logic of his line of thought seems to point directly to the need to develop a theory of the state Yet Clark prefers to formulate the problem in terms of "variables associated with the national society" (p 24) instead of in terms of concepts which may elucidate the nature and purpose of the state's role.

In the chapters which follow some interesting material is presented. Thomas J. Anton writes about what he calls the Swedish machine for providing public services and facilities and tries to explain the absence of politics in its operation. He knows what he is writing about, but he badly needs some concept of the Corporate State to help him make better sense of what he sees. Kenneth Newton tries to make some comparisons between English and American local power structures and notes the importance of

national class division for analyzing the former Various authors manipulate social indicators in a number of local areas in order to show causal linkages This seems a popular activity among community power people The data appear hard, and the results appear scientific. The frequently inconclusive results provide stimulus for further research, and the enterprise gives the impression of being cumulative Jorgen Westerstahl looks at decision making in 36 Swedish communes, Peter Jambrek correlates data from 16 Jugoslav communes, and Michael Aiken and Robert Alford juggle data from some hundreds of American cities which participated in programs connected with public housing, urban renewal, and the War on Poverty These and other studies are all in the "teasing-out-the-important-factors-with-the-aid-of-a-computer" school Perhaps the most ambitious of these is by Robert Fried who correlates socioeconomic variables and performance variables for Swiss, German, and Austrian cities. "The variables were selected partly on the basis of availability and partly on the basis of their presumed relevancy" (p 317) He finds it difficult to come to firm conclusions about the relationship between political party and local outcomes.

In the final section of the book, on theoretical and methodological issues in cross-national research, Mark Kesselman reviews American studies and claims that "many students of American politics assume that, in contrast to virtually every other political system, the United States is exempt from basic conflicts A dominant concern of scholars has been to understand (and, frequently, to foster) consensus and stability, with a corresponding neglect of conflict and change" (p 358) He suggests that befuddled American researchers have either swung to the opposite extreme and overemphasized conflict and national processes or supported a trickle-down theory of power which has assisted those in power to maintain their dominance One cannot help wondering how it is that 20 years of solid research work by, presumably, sensible and solid researchers should have produced so little of lasting importance

The overall impression I get from this book is of muddle and honesty. Variables and factors are juggled, propositions are listed, and arguments are deployed to show that most previous work is of little value One feels that it won't be long before indicators of local outcomes in all the countries of the world will be put on the computer, and someone will argue that level of production and control of the productive forces are "important variables " Capitalism is not listed in the index of this book The attempt to study comparative community politics without an overall framework of political economy seems to me to be a futile exercise The most curious puzzle, perhaps, is why the authors of the articles in this book have remained in their self-confessed confusion for so long

Black Neighborhoods: An Assessment of Community Power. By Donald I. Warren. Ann Arbor: University of Michigan Press, 1975. Pp. xii+194. $9.00.

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Donald Warren (no relation) has written an important book which stresses the substantial differences among black ghetto neighborhoods and uses an analytical framework as a basis for drawing policy implications from these differences. He reports his research findings within a conceptual formulation based on the findings themselves as well as on pertinent literature on black ghetto neighborhoods. These are woven together in a carefully developed exposition which arouses and maintains the reader's interest through the meaning which the data take on within the author's theoretical framework At times, the reader is not quite sure whether a given statement derives from specific findings, from inferences drawn from them, from generalizations made by other investigators, or from basic theoretical materials, but in most cases the source is clear.

The findings are based on a survey questionnaire administered to 932 black and 760 white respondents in 28 elementary school districts in Detroit Sixteen of these districts (which are taken as "neighborhoods") are black, 12 are white. The survey data were gathered in 1969 and 1970.

In this book, as in the other reports from his Detroit study, Warren has utilized a typology of six neighborhood types, varying along a number of specific, empirically established dimensions The distribution of black neighborhoods among these types differs markedly from that of white neighborhoods. More central than this typology for the author's present work is his distinction between homogeneous and heterogeneous neighborhoods, which forms the basis for most of the book's analysis. Warren asserts that black neighborhoods can best be understood in terms of their degree of "heterogeneity, social class diversity, spatial compression, and the interrelations of these factors with one another" (p. 15). Far from being similar to one another, black neighborhoods vary greatly in their extent of heterogeneity and as a whole are more heterogeneous than white ones. The "social compression" which helps shape and structure black neighborhoods produces both extremely homogeneous and extremely heterogeneous neighborhoods, though a more moderate degree of heterogeneity would probably be "the most viable form" (pp. 66, 67).

The analysis of findings proceeds from neighborhood structure to the role of voluntary associations, to formal and informal neighborhood leadership, and to a particularly cogent treatment of alienation and activism in the black ghetto. These, are all considered in their interrelation.

The author has some strong assertions to make by way of policy implications of his findings and analysis. They highlight the complexity of the neighborhood control issue by relating it to the complexity of the neighborhoods themselves. Each chapter makes some explicit policy-oriented

assertions, some relatively minor, some extremely important. Three examples of the latter type: "Policy decisions which suggest increasing the number of indigenous leaders, the utilization of local officers of organizations, and the tapping of informal neighborhood reputational leaders therefore become strategies fraught with danger" (p. 114). "Since voluntary associations are more expressive of common interests rather than diffuse goals, attempts at umbrella forms of community organization must be seriously questioned" (p. 136). "The CAP [community action program, "antipoverty agency"] type of structure seems a viable and needed intervention group for black heterogeneous neighborhoods Nevertheless, its suitability for homogeneous areas is clearly in doubt" (p 143)

Since such inferences are of crucial importance, the reader understandably needs to see their direct relation to the findings on which they are based and, even more important, to the specific indicators which produced the findings. Here the reader is in for a disappointment. Although there is an appendix giving some tables, there is nowhere a clear description of the methodology. Variables are utilized ordinally in the narrative report, with no indication of the magnitude of the differences and with no information on the way they were operationalized and their results summated. No doubt space limitation precluded a full account, but precisely because the findings and implications are so important the reader needs to know much more about what is, operationally, behind the use of such terms as "homogeneous," "heterogeneous," "status inconsistency," and even to what extent a black neighborhood *is* a black neighborhood and a white neighborhood a white one.

There are three bridges between the data and the conclusions, and each one leaves the reader with a knowledge gap One is the bridge between the variables used for analysis and their operational indicators in the questionnaire. Another is between this sample and the universe it purports to represent. (Warren continually speaks in global terms about white neighborhoods, black neighborhoods, heterogeneous neighborhoods, homogeneous neighborhoods, and so on, as though one can generalize from a single city The experience of the Chicago ecologists in generalizing to all American cities from the Chicago data seems not to have daunted him) The third bridge is between the specific findings and their policy implications. Although these implications are extremely interesting, the reader really cannot evaluate them without more knowledge of the way the variables were operationalized and the way their indicators were aggregated

These are important reservations They do not, however, erase the fact that *Black Neighborhoods* is a thoughtful, highly sophisticated, carefully thought out, challenging analysis which students of urban sociology and of the impacts of institutionalized racism cannot afford to ignore Further, despite the lacks noted above, the book represents a happy blend of data, findings of others, theoretical formulations, and policy inferences One cannot say that the author has not milked his data The cream-to-skim-milk ratio is gratifyingly high

Urban Planning and Politics. By Richard S. Bolan and Ronald L. Nuttall. Lexington, Mass.: D. C. Heath & Co., 1975. Pp. xviii+211. $16.50.

Stephen L. Elkin
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The literature on decision making in American communities is notable for sharp debate over methodology and appropriate theoretical starting points. Richard Bolan and Ronald Nuttall attempt to advance the discussion by presenting a large number of propositions which "synthesize" both "structural" and "process" approaches (p. 19) and by drawing on both case material and interview data. They wish, finally, to relate their analysis to the education of urban planners and other analysts The last goal receives rather perfunctory attention; the first provides the bulk of the study.

The book contains an initial brief discussion of the literature on community power and decision making which is then drawn upon in the authors' synthesis. The attempt to support their propositions consists of four very briefly presented case studies of major policy decision in three cities (New York, Boston, and Pittsburgh) followed by an analysis of data obtained in interviews with some of the actors in these conflicts The data consist of reports on the respondents' own activities and on those of other participants. A discussion of how the initial statements may be modified in the light of the data follows; this in turn is followed by some concluding remarks summing up the significance of the research, including its limits.

The major theoretical shortcoming of the study is that its purpose is to analyze the behavior of collectivities while the data considered is almost entirely limited to the correlates of behavior of individuals within them. Now Bolan and Nuttall are aware that they somehow must get from analysis of individual behavior to statements concerning collective action and outcomes. There are two attempts to do so, one of which may not be so intended while the other clearly is. Both reveal the deficiencies of the "model" presented and of their general thinking about political processes. (The case studies which do provide some data on the process of community decision making are not heavily drawn upon.) The first employs stepwise regression to determine the relative contribution of individual characteristics and organizational attributes (which are treated as individual level characteristics by being assigned to individuals) to explaining an actor's reputation for being "important to an issue" (pp. 114–20). Although the authors recognize at one point that this says nothing directly about why, for example, communities decide to build urban freeways or not, later in the book this analysis is said to contribute substantially to an understanding of decision-making outcomes (pp. 127–28).

More interesting is the second attempt, in which the notion of seeing the policy conflicts as a competition between two "teams" is advanced. Each team (those for and against) has a certain level of resources (which is defined without reference to organizational resources that might be used), and the problem is to determine which team is the stronger. This will

enable a prediction of the outcome to be made (pp. 92–98) As a description of what happens such a statement is of little value. And as theory, possibly leading to prediction, it is unlikely to be very powerful. The strength of a team cannot be understood by adding up the attributes of individuals who share a policy position. Do the individuals act in concert (i.e., as a coalition)? How much of their resources do they contribute? What actual and promised rewards hold them together, if at all, etc.? These questions all point to the conclusion that talking about teams as a simple summation of members obscures the central facts about collective action of this kind. Along the same lines, there is no discussion of interaction between the (possible) coalitions Surely some tactics are employed, countermoves made, skills exercised, all of which affect the outcome Again, a process needs to be analyzed. Bolan and Nuttall think of the notion of competition between teams as an analogy (p. 94), and that is precisely the problem. The heart of their theoretical task is how best to understand the various interactions between individuals and organizations which produce the outcomes. Presenting an analogy is a way of avoiding this question.

That Bolan and Nuttall would not be very incisive in trying to join individual behavior to collective action can be seen from their initial discussion of the variables related to collective outcomes. There is no systematic attempt to show how the variables relate to each other or to the resultant policy decisions Effectively, there is an enormous laundry list of possibly important factors (several hundred, depending on interpretation), but little consideration of how the propositions about the behavior of subunits may be linked to statements about the behavior of the whole In general, one may wonder about a strategy of inquiry which consists of enumerating so large a set of variables. A more useful manner of proceeding might be to start with some reasonably simple conception of the kind of political process being investigated and then elaborate as necessary This at least gives some meaning to the idea of using a model.

The data analysis also suffers from a number of deficiencies which make it very difficult to interpret. Many of the difficulties revolve around uncertainty whether the data concerning the behavior of individual actors consist of the aggregated reports of those interviewed or whether each report by each respondent is being analyzed (i e , no summation is carried out and the respondents' reports are in effect the units of analysis as against the actors). If the first alternative holds true, as it appears to, the question of how the scores were created should have been more than barely touched upon. The relationship of the reports of respondents one to another and the relationship of each report to the behavior of the actors pose a central and complex question affecting the validity of most of the data considered. If the second procedure mentioned above was followed, it is not clear what the status of the whole analysis is. This is all complicated by the fact that some of the variables concern only the respondents, who are a subset of all the actors considered. Additional difficulties are present in the analysis of organizational actors. For example, it may be doubted whether the amount of an organization's budget available for contention

over an issue is an adequate measure of whether it has a "significant reward-punishment scheme" (p. 106). It should be noted that the actual size of the budget is not used, but only reports of respondents on whether an organization's budget was larger or smaller than that of their own organization. The same technique is used for measuring other organizational characteristics.

The ultimate purpose of the study, according to Bolan and Nuttall, is, as noted, to improve the education of urban planners and other analysts However, what they say that have learned is in effect largely a repetition of what successful planners at least and many other observers have already noted, namely, that planners work in a complex contentious political environment and that to be effective they must be skilled advocates who understand something of the surrounding political arrangements Perhaps this point is worth repeating, and indeed it may be all that studies of how political processes operate can contribute to the education of planners. If that is so, the hopes that Bolan and Nuttall hold out for educational improvement built on studies of community decision making are misconceived

The Trumpet of Prophecy: A Sociological Study of Jehovah's Witnesses By James A. Beckford. New York. John Wiley & Sons, 1975. Pp. xii+244. $17.95.

Phillip E. Hammond

University of Arizona

About 1870, Charles T. Russell gathered around him in Pittsburgh a handful of sympathetic souls and began a study of the Bible. Their study was of the millennialist variety, grounded in the belief that God's plan for the world unfolds in a manner that can be discerned and that will "culminate" in some dramatic fashion -a cataclysm, a Second Coming, or whatever—which in turn will yield 1,000 years of peace and heavenliness Such an orientation encourages the otherwise rare combination of rationalism and extremism, of nonconformity and public display of that nonconformity; in Russell's case the result was the Watch Tower movement, an organization of Jehovah's witnesses, committed to convincing us all and worldwide in scope.

We know them in their several aspects: as intrepid litigants who have caused the rewriting of church-state law regarding conscription and flag saluting, as persistent doorbell ringers and disputants, as producers and purveyors of an endless supply of literature, and as exceptionally well-behaved citizens (with due exception for the above-mentioned doorstep persistence)

James A. Beckford, presently a lecturer in sociology at the University of Durham, England, researched British Witnesses for a thesis at the University of Reading, and, in consequence, the sociology of religion possesses a fine (not distinguished, but fine) addition to its literature.

The Trumpet of Prophecy divides easily into two parts Chapters 1–5 are largely descriptive, the first three borrowing from historical accounts, the fourth and fifth outlining the various positions, career paths, activities, and doctrines of present-day Witnesses. In these chapters Beckford follows a pattern that is essentially chronological with little forays into abstraction, testing the applicability (not really the adequacy) of one or another theoretical assertion. Thus, we learn that various theories of religious behavior, sometimes correctly and other times incorrectly, anticipated the Watch Tower movement. Nothing much is gained by these forays, but nothing much is ventured, the whole exercise being probably a symptom of dissertationitis. Apart from these bits of fluff, however, the first part of *The Trumpet of Prophecy* introduces well the book's subject matter

It is the second part (chaps. 6–10) that advances our sociological understanding of Jehovah's Witnesses. Midway through the book, Beckford asserts that, contrary to most thought perhaps, "the millenarian message is so highly ambiguous that it is largely determined by the organizational form dictated by the founders or members of the groups that carry the message" (p. 119). Not only does this assertion provide warrant for all the preceding descriptive chapters, but it also offers a context for the theoretical questions these latter chapters address, namely, why persons are attracted to and remain in such a movement as the Watch Tower movement. The answer, briefly, is seen in its unique organization.

Beckford organizes his discussion of these matters around three "theories". (1) frustration-compensation, or the notion that real or imagined status deprivation leads to sect involvement; (2) world-view construction, or the notion that social and moral confusion encourages interest in extremist religious views; (3) social solidarity, or the notion that social isolation predisposes people to sectarianism

Regarding the first, Beckford devotes all of chapter 7 to a careful analysis and finds almost no support. While it is true that Jehovah's Witnesses include no persons from either extreme of the social ladder, they do exhibit great status variation Moreover, there is little evidence of their greater or lesser mobility, of their greater or lesser relative deprivation. And no relationship exists between economic change and the vitality of the British Watch Tower movement In sum, "none of the hypotheses from the most celebrated theoretical tradition in the sociology of religion is able to shed much light on the reasons why people became Jehovah's witnesses in Britain" (p. 158).

The second theory fares somewhat better. Beckford found in interviews that with "remarkable uniformity" Witnesses claimed that their conversion to the movement took place amid mental and or physical anguish. But wisely he cautions qualification. The Watch Tower teaches that the world is on a collision course, so it is not surprising to find persons recollecting their own attraction to this message, even if—in a "vocabulary of motives" sense—it merely enables them to impute meaning to their own histories. The fact is, most Witnesses are recruited by door-to-door evan-

gelism, and while recruits typically must be predisposed to the ideology, predisposition alone is clearly not sufficient.

That brings up the third theory Are Witnesses social isolates, and does isolation lead them to involvement? Well, yes and no. Generally speaking, converts report no prior membership in voluntary groups or other intermediate relations outside of kin, but they also report highly satisfactory family ties. Members describe contacts at church as warm and friendly, but, says Beckford, services are "extremely arid," and it is difficult to believe that persons desiring social solidarity would select the Kingdom Hall in preference to "the emotionality or piety of other non-conformist groups" (p. 179). Moreover, solidifying one's position as a Witness is a long, arduous task; neophytes must prove themselves before full acceptance.

Thus Beckford more or less rejects all three theories as inadequate to explain people's conversion to the Watch Tower movement In their place he offers an "accounting" scheme of five stages. The first two (general predisposing conditions) are beyond the control of Witnesses. The other three, though, are very much influenced by the aims, structure, and values of the Watch Tower society itself: (1) Doorstep evangelists can precipitate a "crisis" by offering an interpretation of the world and the divine plan for it. (2) Mandatory field service by all recruits (but never singly) serves to augment and confirm initial conviction. (3) A program of everexpanding responsibility binds persons to the movement increasingly even as it severs more and more outside contacts.

The result? "Affiliation to the Watch Tower movement must be considered as a *process* determined by, and controlled according to, the deliberate policies of Jehovah's witness personnel" (p. 187; my italics). The purpose of the earlier descriptive chapters is now clearer. Beckford is reminding us that, as in the analysis of any choice behavior, actors not only are propelled by their own characteristics but also are influenced by characteristics of the movements to which they are propelled. This volume is a fine case study of that proposition.

The Obsolete Necessity: America in Utopian Writings, 1888–1900. By Kenneth Roemer. Kent, Ohio: Kent State University Press, 1976. Pp. xiv+239. $10.00.

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> And when we pick up one of these volumes, which usually hasn't been checked out in years, and come across blank membership forms, phony subscription lists, or appeals that were never heeded, it is almost impossible not to pity the utopian authors. [P 178]

Kenneth Roemer's study of more than 150 of these dusty volumes provides a useful compilation of the major issues and themes of late-19th-century

American utopian literature. The sociology of literature is in need of descriptive studies such as this, for they provide the raw material by which others, more theoretically minded, can undertake analyses which attempt to investigate the social origins of literary movements. Roemer's book does make occasional efforts at such a sociological analysis, but on the whole the value of the work lies in its presentation of material from his extensive research

The utopian novels written between 1888 (the publication date of the single most important one, Edward Bellamy's *Looking Backward*) and 1900 were a literary phenomenon of major importance Erich Fromm, in a forward to recent editions (New York· Signet Classics) writes that "the book attracted and deeply influenced men like John Dewey, Eugene V. Debs, Norman Thomas, and Thorstein Veblen it is one of the few books ever published that created almost immediately on its appearance a political mass movement Between 1890 and 1891, 165 'Bellamy Clubs' sprang up all over the U.S., devoted to the discussion and propagation of the aims expressed in *Looking Backward* "

Many of the utopian writers were political activists, others were reform journalists. There is no question but that some of the specific utopian elements of the books entered the political arena, particularly through the Populist movement Utopian communities were actually founded; the Kaweah Utopian Community in California used to advertise in the *Nationalist*· "Bellamy's Dream Realised" (p. 43)

Bellamy's book sparked an outpouring of other utopian novels; over 200 were published in the following decade The *Nationalist*, a journal inspired by *Looking Backward*, had a circulation of 69,000 in 1890, while Bellamy's own journal, the *New Nation*, and a dozen other periodicals spoke from the same perspective. In short, the utopian novel dominated American literature for a decade (p. 2). Roemer spends little time investigating why these novels became so popular. He does cite Lewis Mumford's opinion that utopian literature thrives during turbulent transition periods (p 4), and no one can deny the turbulence of the epoch in question. The country was shaken by the panic of 1873, the violent strikes of 1877, the Homestead steel mill shootout of 1892 with the industrialist Henry C Frick leading the battle against the unions—and the anarchist Alexander Berkman's attempted assassination of Frick--not to mention the stock-market crash of 1893. In 1896 one-eighth of the American population owned seven-eighths of the wealth of the land; of the wealthy one-eighth the top 1% owned more than the remaining 99% It is against this background of starkly visible class struggle that one must understand both the radical and escapist aspects of utopian literature.

On the whole the utopian authors were inexperienced writers (p. 141); the awkwardness of their contrived characters and situations makes this evident. The greatest exception is of course Mark Twain who penned the major work *A Connecticut Yankee in King Arthur's Court* (1889). The writers tended to be middle- and especially upper-middle-class professionals; they had a remarkable rate of college attendance—72% (p. 10).

When compared geographically with novelists in general at the time, they were only half as likely to live along the Eastern Coast and were three times more likely to come from the Midwest or West.

Critics of utopian literature abounded The *Literary World* reviewers complained that "utopian fiction was 'all political economy'" (p. 109). Indeed the principal bogey men of the utopians were greedy capitalists, corrupt politicians, and the lawyer class (p. 145). About 85% of the utopian works denounced capitalism as a "wisely calculated and prudently guarded system for the outlet and satisfaction of egotistic lusts" (p. 77). The utopians clearly felt that too many people were suffering from oppression and poverty (p. 18), and that the American laissez-faire system was in fact becoming a jungle of Pinkerton-supported monopolies and trusts (p. 90) Perhaps because the writers analyzed the chaotic inequality of their age in terms of a threat to the development of the ideal individual (p. 89), their call for a cooperative society was expressed more in terms of "Altruism," "Cooperative Individualism," and "Nationalism" (p. 77) than in terms of socialism. The Christian influence on the movement was of major importance; some writers called for a "Christian Socialism," Bellamy called his Nationalism an "intensely Christian Movement," and Thomas L Harris labeled his socialism "Theo-Socialism" (p 88). Twain and several other authors envisioned bloody revolutions; most authors were less explicit in picturing a general "era of violence." The most common literary image of the oppressed multitudes and their future reaction was the volcano (p. 22). Over and again this is used as a metaphor for the explosive frustrations of the poor.

The utopians called for extensive, systematic changes (p. 91). Some foresaw the abolition of money, others merely wanted it confiscated from the wealthy. About a fifth advocated socialism, even if in other terms Some supported a government with decisions made by "disinterested experts," while others wanted a rejection of the American tradition of stopgap economic reforms (!). As one utopian put it, "It was not one evil we wanted to correct . . We wanted a general improvement" (p. 134). The utopian novels remained thoroughly American; all but eight put the setting of their ideal societies in the United States (p. 47).

The utopian imagination spread in many directions. Perfect cities of the future were outlined and diagramed (p. 153), while wonderful machines were seen to help eliminate the gap between the rich and poor. Bellamy writes of a system of mechanical umbrellas that cover all the sidewalks of Boston when it rains. Education was reformed, often along lines of technical and manual training, as was possible in utopia since the skills involved were no longer associated with menial labor (p. 123). Various utopian reforms of education were later to appear in John Dewey's programs of "progressive education." Marriage came under heavy attack, as in the work of Lester Ward, and Thorstein Veblen; many utopians wanted to see women emanicipated from the kitchen and men emancipated from a life of mere money making (p. 130). One novel which advocated easy divorce was roundly criticized as proposing "sexual communism" (p. 131).

Many utopians foresaw a United States without a government—the new harmony made it obsolete. Others envisioned a loose form of worldwide community of nation states (p. 143).

Roemer's study pictures the utopian novels as expressing two principal thrusts. On one hand the authors—and no doubt many of their millions of readers—had lost faith in American capitalism (p. 77), the redemptive quality of virgin land (p. 46), and the Horatio Alger myth (p. 57). On the other hand the society they dreamed of was perhaps more influenced by Christian than by socialist notions The utopian authors did indeed call for a new economic system, but in their heart of hearts what they dreamed of was a Christian utopia (p 96).

The utopian novels reflected with an almost naive accuracy the chaotic class struggles and rampant social change of their era. Roemer's useful study and the literature he cites in an extensive bibliography make this material available for analysis from the still underpracticed subdiscipline of the sociology of literature Is this genre of literature thoroughly passé? Roemer's title suggests that utopian writing is both obsolete and necessary. He cites the great sociologist Lewis Mumford (p 171) who wrote that "we can never reach the points of the compass; and so no doubt we shall never live in utopia; but without the magnetic needle we should not be able to travel intelligently at all."

The Classless Profession: American Schoolmen of the Nineteenth Century. By Paul H Mattingly. New York New York University Press, 1975. Pp. xxiii+235. $15.00.

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It is of course a truism that modern society is qualitatively and quantitatively different from other types of societies Most scholars, irrespective of disciplinary affiliation, attribute these differences to the need to create new structures to deal with the social dislocations caused by rapid economic and technological change. Indeed, a number of distinguished historians, including Alfred D Chandler, Jr., and Robert Wiebe, have periodized American history not by a familiar political chronology based upon key presidential elections, but by a chronology that divides the development of American society into preorganizational and organizational phases

The organizational phase has a number of distinguishing characteristics One of the most important is professionalization, which represents an effort to introduce order and stability into a large, mobile, and complex society undergoing continuous change Before the 20th century, medicine, law, and religion were the only professions (implying that the very meaning of "profession" has undergone fundamental changes); in modern society professions are so numerous that any effort to enumerate them proves difficult. Curiously enough, most historians and social scientists have long

been aware of the significance of professionalization but have not undertaken a systematic analysis of its origins and evolution. Although a great deal is known about its consequences, relatively little is known about the precise conditions that were responsible for both its development and particular configuration.

In *The Classless Profession* Paul H. Mattingly attempts to delineate the professionalization process in American education during the 19th century. Using several key organizations, representative individuals, and a generational analysis, he seeks to explain how the behavior and ideology of 19th-century schoolmen could lead to the adoption of certain bureaucratic structures and give rise to a pragmatic stance that left basic assumptions and conclusions about the profession untouched by critical and rigorous self-examination.

Mattingly begins with an important idea, namely, that the notion of "character" was central to the professionalization of schoolmen. Implied in this thesis—though never explicitly stated—is the idea that the meaning of professionalization underwent fundamental changes during the 19th century. "Character" referred initially to "a moral potential within each person and was somewhat susceptible to improvement and refinement given the proper influences" (p. xii) Over a period of time, however, its inner meaning was altered by the changing class origins of schoolmen To the first generation considered (1830–60), character meant specific attitudes of intellectual discipline and self-possession. The function of education was to cultivate character and produce socially responsible persons who were neither politically biased nor partisan; the teacher was in effect an evangelical minister charged with creating an "awakening" among the American people Left untouched were fundamental questions about what constituted social responsibility, the issue was buried beneath a professional rhetoric that "gave the impression that true teachers confronted the essential moral facts of life" (p. 56)

The second generation of schoolmen (1860–90), on the other hand, shifted the focus by separating the concept of "awakening" from education and emphasizing a form of professionalization. Under their aegis the teacher became less of an evangelical minister and more of a professional with technical training in pedagogy. Teaching as a profession became available—at least in theory—to all who were receptive to educational improvement and instruction, first in teachers' institutes and then in normal schools. These changes reflected a shift away from an essentially upper-class ideal of education held by the first generation toward the lower-middle-class ideals subscribed to by their successors. In the final section of his book Mattingly traces the decline in the authority of the normal school and the rise of professional policymaking in the form of the school superintendent's role.

While the problems raised by Mattingly are significant, his answers are vitiated by an appalling lack of clarity and a way of employing language that serves only to obfuscate the subject and confuse the reader. Stripped of its ponderous language and curious organization, his book contributes

much less than it could have to an understanding of the process of professionalization in an individual occupation. There was more to this process than the author assumes: structural changes in society and the nature of the student population surely played some role. And despite the inclusion of a series of quantitative tables (which are never integrated into the text), the author tells us surprisingly little about the teaching profession in 19th-century America. Perhaps I am being unfair; if so it is due largely to a style of writing and organization that is at times confusing.

Beyond this, however, lies a far more important conceptual issue. It is possible to explain the professionalization of any field in terms of internal factors Such an approach, while illuminating the evolution of individual professions, will contribute little toward an understanding of professionalization in social and aggregate terms For if professionalization cut horizontally across American society (as it assuredly did), other approaches—especially comparative and structural ones—may prove more fruitful in explaining the circumstances that transformed America and other Western nations into modern societies. In this sense Mattingly's study of 19th-century schoolmen suffers from a form of parochialism insofar as the larger problem of professionalization is concerned.

College Professors and Their Impact on Students By Robert C Wilson, Jerry G Gaff, Evelyn R Dienst, Lynn Wood, and James L Bavry. New York: Wiley Interscience, 1975 Pp. xi+220. $12 95

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Despite the vast amount of research on the general impact of college on students, relatively little is known about the specific nature of student-faculty interaction and about the conditions which favor informal contact outside the classroom This unusual study (actually two studies) provides much information which is fresh, interesting, and provocative On the basis of a survey of faculty members in six institutions and a second survey of students and faculty members in eight others the authors reach four broad conclusions about when faculty members do "make a difference in the lives of students." First, they argue, some faculty do indeed affect some students' lives in a substantial way, perhaps even to a greater degree than imagined by previous authors, particularly Feldman and Newcomb in their massive *Impact of College on Students* (San Francisco Jossey Bass, 1969). Second, not all students use faculty effectively: there is a great deal of variation in the degree to which students leave themselves open to or seek out faculty influence. Like students, some faculty are more interested and effective in a student-faculty interaction than others. The authors argue that clear social psychological factors distinguish faculty members who are approachable both inside and outside the classroom from those who are less so. A third finding is that faculty appear to derive as much

personal satisfaction from interaction with students as do their students. Finally, some college settings nurture faculty-student relationships more successfully than others. As one might expect, small colleges, or cluster colleges within large universities, engender more faculty influence and outside-class student-faculty contacts than does the University of California at Berkeley.

These findings are based on two surveys. The first went to faculty members at six schools ranging from the University of California at Davis to Hofstra University and a San Francisco area community college. The second study, which combined a longitudinal survey (freshman to senior year) of students with a faculty survey taken in the senior year (1970) came from a similar mix of eight schools including the University of California at Berkeley. The second survey is unusual. faculty members were asked to name the students they felt they had influenced and to name colleagues they felt were effective teachers. At the same time, students were asked to name faculty members who had a strong influence on them. Thus it was possible to look at the characteristics of faculty members who either were nominated by others as effective teachers or considered themselves to be, and to look at the characteristics of students who reported themselves or who were nominated by faculty as having been influenced.

The book, while a report of research, is also intended as a recipe for change; indeed, the last chapter contains a rather extensive set of recommendations for improving undergraduate education. Since the authors are writing for an audience not necessarily trained in statistics, research results are presented in the simplest form possible. Almost all of the findings are based on bivariate cross-tabulations and a few correlations. Despite these limitations (and some rather vague table headings), a good deal of basic information is conveyed. Not infrequently, and refreshingly, one finds a set of negative findings reported in the text. For example, we are told that teachers nominated by faculty and students as being particularly effective differ very little from their colleagues on a wide range of attitudes and values which the authors had assumed to be important in identification with young college students In general, the book succeeds in presenting a great deal of basic information in an effective way.

All of this is not to say that the book is faultless. Although the findings are interesting both the sample and the analysis are flawed to the point that one is reluctant to generalize from the results to any particular kind of school or type of individual. The sample can only be described as haphazard. Although the schools chosen represent adequately the range of school types, the resulting distribution of students and faculty does not generalize to any well-defined population. Although one small table in the appendix compares three faculty characteristics with results from two other studies with favorable results I remain unconvinced. Part of this problem could be dealt with by reporting the results within school but in their desire to make the results understandable to the layman the authors have generally reported bivariate relationships aggregated across school. When the results are reported within school, important individual charac-

teristics of students and faculty are ignored. The bivariate nature of the analysis also makes it impossible to judge the relative importance of variables or to estimate the nature of interactions with such variables as age, sex, and race

It can be argued that deficient data will not support complex analysis. At the same time we must be more careful in drawing conclusions from poor samples than from good ones, and such care usually requires more attention to analytical detail Despite its limitations as a policy document the book is a very useful contribution to the literature and will serve as a reservoir of hypotheses for future research An administrator wishing to use it as a basis for change, however, must carefully examine how the students at his or her school differ from those in the sample Unfortunately, making that determination on the basis of the data provided in the book will be quite difficult.

Education, Equality and Society Edited by Bryan R. Wilson. New York. Barnes & Noble, 1975 Pp 207 $15.00.

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No issue has proved more vexing to educationists, both inside and outside the academy, than the meaning of equality in education. Typically, the fact that equality of *something* is to be prized goes unquestioned, even by those who wonder with Christopher Jencks whether education is not a marginal item on the agenda of an equalitarian society. But equality of what: Equality of resources? Equality of achievement, either between individuals (every child as intellectually competent as his or her neighbor) or across racial and class groups? Or equality of opportunity, a concept which calls to mind the metaphor of a race whose contestants start at the same point?

Since the publication of the *Equal Educational Opportunity Survey* in 1966 we have learned a great deal more about the feasibility of achieving any of these objectives (and the myriad subobjectives that each spawns) and are in a position to guess the probable consequences of policy change, at least at the gross level. But with that knowledge has come a diminution of certainty that any particular course of governmental action is right The more we know, the less we seem willing collectively to impose a particular vision of education on our children· the continuing appeal of education voucher proposals, which substitute consumer sovereignty for a single public wisdom, attests to that

In such an environment, a book such as *Education, Equality and Society,* which (while focusing particularly on English education) calls into question the rightness of the equality premise itself, could have made a useful contribution to our rethinking the matter. The promise of the

volume, as set forth by its editor Bryan Wilson, is considerable: "[T]he case for complete equality in education is shown to rely on inherent weaknesses of philosophical argument, to depend on fallacious psychological assumptions, to have unintended and deleterious cultural consequences, and to face insuperable problems to its practical realisation" (p. 37). But as is so often true of editors' claims, the essays themselves generally do not sustain the hope *Education, Equality and Society* disappoints: it is thin, inconsistent in argument, remarkably insular (the most contemporary American who figures prominently in these pages is James Conant), almost entirely unaware of the past decade's relevant empirical work, and unabashedly snobbish.

The book is a curious anachronism. At points, it reads as if William Graham Sumner had come suddenly upon the full bloom of the 20th century and, unsurprisingly, found it wanting. "Women's so-called Liberation" (p. 21) is dismissed with a passing wave to biological imperatives; the virtues of aristocracy are said to find rare praise these days (p. 130); a concern for cultural differentiation is treated as "a current version of pastoral: . . in the eighteenth century there was a fashion for . . . shepherds and shepherdesses; today they appear in dungarees" (p 154)

What perturbs here is not that the views are not trendy—scholars are not supposed to be trendy—but that they reflect analytical myopia. Philosopher J. R. Lucas concludes his essay with the hope that "even equalitarians" may be taught "to think" (p 61). If there exists such a need, this volume does not fill it. In the book as a whole, the meaning of the concept under assault—equality—either goes undefined or, simplistically, is equated with sameness Individual efforts are not much better The late Sir Cyril Burt, for example, speaks of the inherited and measurable differences in innate, cognitive, and general knowledge (p 84), without reference either to the criticism of the biological primary approach or—more pointedly, in a book treating with educational policy—to the relevance of innate differences in intelligence to an individual's life chances. J R. Lucas, who usefully treats justice for children as not fully describable in either egalitarian or meritocratic terms, but as a question of children's particular "deserts," finds himself defining the requirements of justice in terms of justice (p. 54), a circular and not very helpful exercise. Rhodes Boyson, formerly a headmaster and now a member of Parliament, exaggerates the ills of the British comprehensive schools and reveals a cavalier disregard for the canons of research (students' performance in the schools is compared without reference to socioeconomic backgrounds, to cite but one example); as a consequence, the important things that he has to say—concerning, for instance, the revival of homogeneous grouping in comprehensive schools—are lost in argument.

Lurking beneath all of this are vital questions: the viability of the concept of a received culture; the maintenance of legitimate authority in the school setting (threatened by equality, and even more by demands for students' "rights"); the importance of choice in matters educational; the deadly leveling hand of bureaucracy. But "lurking" is unfortunately the

right word. The issues never become sharp and do not receive the full-dress argument they deserve If the volume was conceived as a response to Michael Young's classic *The Rise of the Meritocracy* (Baltimore: Penguin Books, 1961), or as exposing the considerable dilemmas of socialist education, it fails badly. *Equality, Education and Society* will be comfortable reading for those who share its biases and comfortably ignored by those who disagree with them

At the very end of the volume, Rhodes Boyson notes that the British Labour party, out of concern for a particular kind of egalitarianism, may seek to close all nongovernment schools, compelling parents to send their children to the state system. That policy proposal sharpens debate considerably. An assessment of it might have served as a useful starting point for a thoughtful analysis of equality policy Perhaps someone else will undertake that badly needed task.

Fertility and Deprivation A Study of Differential Fertility amongst Working-Class Families in Aberdeen By Janet Askham. New York and London: Cambridge University Press, 1975 Pp vii+188 $14 50

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Janet Askham's study on fertility differentials among two groups of working-class women in Aberdeen, Scotland, verges on being a classic in the genre. The women in this study are divided into two groups (skilled working class, Class III in the British Registrar General's classification, and manual workers, Class V in that same listing) and further subdivided into women with small families (two children at the time of interview) and those with large families (four and more children) Each woman was interviewed at least three times to obtain data on her background, her fertility experiences and expectations, her plans for the future, and what have been called KAP measures: data indicating her knowledge about, attitudes toward, and practice of contraception.

It is a classic study in that it tests all the classic ideas about fertility differences and does so with the scrupulous and thorough scholarship we have come to expect from British social scientists in the field It differs from past KAP studies in that Askham chose to investigate a small number of women in detail rather than, as is typical of KAP studies, studying large numbers of women via survey research However, the problem with the present study is precisely the problem with the genre as a whole: Askham tests essentially the same hypotheses that the surveys have tested and finds, like the surveys, that relatively few of these hypotheses explain much in the way of fertility differences. She inadvertently demonstrates, I think, that the KAP paradigm has been wrung dry. The three decades of these surveys in the United States and elsewhere have outlined the gross social

parameters of differential fertility, and the time has come to fill in the gaps in our knowledge with a finer brush.

One senses that this study was originally designed to fall into the middle ground between KAP surveys and the kind of intensive, open-ended research typified by Rainwater (*And the Poor Get Children* [Chicago, Quadrangle Books, 1960]). However, the dominant themes here are still the KAP ones, and that is a pity. As Askham herself demonstrates, neither prior education, prior "deprivation," knowledge about sex and pregnancy, family size preferences, nor even knowledge about contraception adequately explains the differences in family sizes in her four groups of women. In addition, the study design itself demonstrates that social class per se cannot account for these differences either. The most fascinating part of this study in fact is the consistent finding across classes that women with small families resemble each other more on almost every measure than they resemble their class peers with large families. For example, the women with larger families, regardless of class, were more likely to marry young, were more likely to be premaritally pregnant, were less likely to know what sexual intercourse entailed, and were slower than their small-family peers in both classes to move to any form of contraception after marriage and later to move to the more effective forms of contraception Each of these differences was more noticeable among Class V women with large families than Class III women with large families, but it is still interesting to note that Class V women with small families were generally better informed than their higher-status sisters with large families.

What these findings mean and how they fit into a causal model is not something that can be answered by the kind of large-scale variables that are investigated in the typical KAP model and have been adapted to the present study. However, to the extent that Askham chose to follow the Rainwater model and obviously had good rapport with her respondents, the long, verbatim accounts presented here begin to raise the questions that social scientists must deal with in order to explain differences in contraceptive use and therefore in fertility. How, for example, do we account for the differences between women who do not use contraception because they find it too "scuttery" (messy) (p. 85) and those who find it equally unpleasant but chose to use it "resignedly" (p. 85)? As another example, Askham presents a respondent who says, "It [contraception] is the lesser of three evils –sterilization, another baby or the pill" (p. 87). Obviously some of the differences are due to the contraceptive method itself (the pill, for example, is perceived by these women as "clean and certain" although not without worries about its safety), but some are due to how these women—and their social context—define the costs and benefits of motherhood, femininity, and pregnancy. A prime example is the woman who tells Askham that being pregnant is a dilemma for her since her husband is much nicer to her when she is pregnant and she likes going to the maternity hospital for "a rest and some good food," but that children bring money problems and problems of responsibility (p. 90). We need a new model to

explain how women like these engage in the decision making that lets them weigh the various life options they see open to them.

Once the decision is made to investigate how these women negotiate their social worlds, by the way, it becomes clear that there is at least one other group of people who are salient members of that social world and who are rarely studied in their own right: the husbands. One of the few social-demographic variables that did seem to differentiate large families from small in this study was that husbands of large families in both classes were more likely to be ill, disabled, or unemployed for long periods of time. While we don't know whether these events in men's lives lead to, or result from, higher fertility, they are certainly worth exploring

This is not to say that Askham is totally unaware of the social-psychological and decision-making aspects of differential fertility. On the contrary, she discusses them in a wide-ranging first chapter as well as including them in a flowchart which is one of the most sensible I have seen, although I would assert that there are more feedback loops among the variables than Askham outlines. But the reluctance to make a wholehearted commitment to these "first-person" variables is due to Askham's assumption, which is widespread in social science, that researchers cannot ask respondents how they ended up in a particular social category.

It is true that to do so can create some knotty theoretical problems. As Cicourel has pointed out ("Fertility, Family Planning and the Social Organization of Family Life: Some Methodological Issues," *Journal of Social Issues*, vol. 23 [October 1967]), it is not clear that respondents always share the same frame of reference as researchers even when they are ostensibly speaking the same language Further, what the KAP studies have demonstrated is that people often make family size decisions (or nondecisions) in response to social, demographic, and economic influences of which they are routinely unaware But the legacy of the KAP studies is precisely that they can serve to make us sophisticated listeners: when we ask individuals to describe the "social maps" that they use in order to negotiate their options about fertility, we are aware of forces that they don't know about. We know the broad-scale effects of ethnicity, SES, education, religion, and the like, although these may not necessarily be conscious or salient factors for the individuals concerned. Thus, while we need to go beyond merely fitting people into the KAP model, I am not suggesting that we simply allow first-person accounts to stand unquestioned What we need to do is mesh the survey research data with data from intensive interviews in order to explain how both the social structure and the individual's perception of it act together to influence behavior. What will result is a richer and more productive sociology of fertility. Had Askham chosen this path, we would have had a groundbreaking book instead of a thoroughly competent reworking of heavily tilled fields. Perhaps Askham can be persuaded to reexamine what appear to be priceless data in the light of these other perspectives on fertility research.

parameters of differential fertility, and the time has come to fill in the gaps in our knowledge with a finer brush.

One senses that this study was originally designed to fall into the middle ground between KAP surveys and the kind of intensive, open-ended research typified by Rainwater (*And the Poor Get Children* [Chicago, Quadrangle Books, 1960]). However, the dominant themes here are still the KAP ones, and that is a pity. As Askham herself demonstrates, neither prior education, prior "deprivation," knowledge about sex and pregnancy, family size preferences, nor even knowledge about contraception adequately explains the differences in family sizes in her four groups of women. In addition, the study design itself demonstrates that social class per se cannot account for these differences either. The most fascinating part of this study in fact is the consistent finding across classes that women with small families resemble each other more on almost every measure than they resemble their class peers with large families For example, the women with larger families, regardless of class, were more likely to marry young, were more likely to be premaritally pregnant, were less likely to know what sexual intercourse entailed, and were slower than their small-family peers in both classes to move to any form of contraception after marriage and later to move to the more effective forms of contraception Each of these differences was more noticeable among Class V women with large families than Class III women with large families, but it is still interesting to note that Class V women with small families were generally better informed than their higher-status sisters with large families.

What these findings mean and how they fit into a causal model is not something that can be answered by the kind of large-scale variables that are investigated in the typical KAP model and have been adapted to the present study. However, to the extent that Askham chose to follow the Rainwater model and obviously had good rapport with her respondents, the long, verbatim accounts presented here begin to raise the questions that social scientists must deal with in order to explain differences in contraceptive use and therefore in fertility. How, for example, do we account for the differences between women who do not use contraception because they find it too "scuttery" (messy) (p. 85) and those who find it equally unpleasant but chose to use it "resignedly" (p. 85)? As another example, Askham presents a respondent who says, "It [contraception] is the lesser of three evils—sterilization, another baby or the pill" (p. 87). Obviously some of the differences are due to the contraceptive method itself (the pill, for example, is perceived by these women as "clean and certain" although not without worries about its safety), but some are due to how these women—and their social context—define the costs and benefits of motherhood, femininity, and pregnancy. A prime example is the woman who tells Askham that being pregnant is a dilemma for her since her husband is much nicer to her when she is pregnant and she likes going to the maternity hospital for "a rest and some good food," but that children bring money problems and problems of responsibility (p. 90). We need a new model to

explain how women like these engage in the decision making that lets them weigh the various life options they see open to them.

Once the decision is made to investigate how these women negotiate their social worlds, by the way, it becomes clear that there is at least one other group of people who are salient members of that social world and who are rarely studied in their own right: the husbands One of the few social-demographic variables that did seem to differentiate large families from small in this study was that husbands of large families in both classes were more likely to be ill, disabled, or unemployed for long periods of time. While we don't know whether these events in men's lives lead to, or result from, higher fertility, they are certainly worth exploring.

This is not to say that Askham is totally unaware of the social-psychological and decision-making aspects of differential fertility. On the contrary, she discusses them in a wide-ranging first chapter as well as including them in a flowchart which is one of the most sensible I have seen, although I would assert that there are more feedback loops among the variables than Askham outlines. But the reluctance to make a wholehearted commitment to these "first-person" variables is due to Askham's assumption, which is widespread in social science, that researchers cannot ask respondents how they ended up in a particular social category.

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The Reconstituted Family: A Study of Remarried Couples and Their Children. By Lucille Duberman. Chicago: Nelson-Hall Publishers, 1975. Pp. xv+181. $11.00.

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Most sociological studies of American family and kinship can be characterized as pedestrian at best. With a few exceptions, notably Bernard Farber and William Goode, sociologists studying aspects of what they are quick to proclaim is *the* basic institution, have done so with little imagination or technical facility. For some reason— perhaps because in American society the family, though presumed basic, has been associated with domesticity and *only* domesticity—it would appear that the more perceptive of our colleagues have characteristically chosen other areas of the social to analyze and illumine. In any case, however one wishes to account for it, the sociological literature on American family and kinship yields little more than half-tested theory and untested opinion presented, more often than not, as empirical generalization

I subscribe to the notion that social controversy is more likely than anything else to be generative of interesting sociology Primarily because of the neofeminist critique, the American family has become the focus of considerable controversy in the past few years; and so I have been expecting the intellectual state of affairs which I have just described to begin to change for the better I was thus hopefully expectant when I read *The Reconstituted Family* by Lucille Duberman. Certainly if what Farber has written about *permanent availability* is correct, then the phenomena of the marriage of the formerly married and the character of the new nuclear families which they and their children constitute will become increasingly significant in any empirical consideration of the normative and behavioral dimensions of American family and kinship. And if this is so, a book purporting to "provide professionals, academics, and interested laymen with fresh insights concerning the reconstituted family" (quoted from the book's dust jacket) might be expected to have something important to say about phenomena which are both theoretically and empirically significant. I am sorry to have to report, however, that as far as Duberman's efforts are concerned my hopeful expectations have been disappointed She has written a book which serious students of the family can neglect with no fear that they are failing to keep up with the literature. I have come to this admittedly ungenerous conclusion because of the following:

1. *Duberman's sample is a nonrandom convenience sample which renders her findings inadequate as a basis for even the most tentative generalizing.* Her title itself, *The Reconstituted Family,* implies a general treatment which a sample of 88 (why 88? why not 100 or 95?) families drawn in Cleveland, Ohio, *cannot* generate. Although throughout the book there are the usual disclaimers regarding generalization, it is clear that Duberman wants to draw the reader's attention to characteristics which are

presumably general to the reconstituted family. We are told that "the object of this study is . . . to explain the dynamics of the reconstituted family, its possible effects on society and the societal influences upon it." Unfortunately, 88 families from Cuyohoga County, Ohio, just won't suffice for so admirable a purpose. What we have between these covers is a description, more or less (sometimes *more* because the author has no compunctions about quantifying her own judgments and very frequently *less* because her data on significant areas of familial interaction are often nothing more than the reports of one-half of each interacting dyad [chap. 5, "Stepparent-Stepchild Relationships," and chap. 7, "Outsiders' Attitudes"] or reports of third-party observers [chap. 6, "Stepsibling Relationships"]) of 88 families residing in one community. This of course is pretty standard fare for sociological research on the family, but the author should expect no praise for conforming to a standard which is inadequate to begin with Frankly, I am not interested in 88 families in Cleveland unless I have some reason to believe that their experience is generalizable; and Duberman, despite her claim that Cleveland was chosen because "it is a 'middle' American city, both geographically and emotionally" (p. 9—*this is presumably an indication of generalizability, although I am not* sure to what universe), has not given me a reason to do so If, moreover, it is true, as Duberman claims, that the personal nature of the problem, the difficulties she encountered in conceptualizing her study, and the paucity of previous research led her to choose so limited a sample (I await, perhaps in vain, the day when this facile rationale for an inadequate sample will no longer be invoked'), I can only conclude that she is premature in publishing. Pilot studies (and at best this is a pilot study) can be useful, but only as first steps to further research. Family research, it seems, is aclutter with pilot studies treated as finished products. Duberman has simply added to the clutter.

2. *Duberman's treatment of the collected data is often idiosyncratic and incompetent.* There are a host of problems with the author's analysis of her data. Space does not permit detailed comment on them all, but any prospective reader should beware of difficulties such as the following. Duberman uses "scores" which are arbitrarily derived and incompletely described. What shall we make of a Family Integration Score which apparently (we are never told just how the score was derived) adds together husband's judgments of family closeness, wife's judgments of family closeness, and joint husband and wife judgments about "the family as a group," as well as ratings by the author herself, and is then subdivided into three categories, Low, Moderate, and High? Are all the data used to "construct" the score equivalent?What is the justification for adding self-perceptions and researcher's ratings? What we have is a score whose validity is highly suspect to say the least. If this were but one scoring problem it might be ignored, but unfortunately the author is consistently incompetent. We have the same problem with the Husband-Wife Relationship Score and the Parent-Child Relationship Score.

Duberman tells us that her study is descriptive. This is her rationale

for the type of sample she has chosen. Nevertheless she persists in treating her scores as though she were testing hypotheses (see chap. 3, pp. 24 ff.; chap. 4, pp. 44 ff.; chap. 5, pp. 55 ff.; chap. 6, pp. 69 ff.; chap. 7, pp. 81 ff.), as though they indexed behavioral outcomes which could be predicted on the basis of some theory which she has developed and wishes to test. Aside from the fact that no real theory informs this study (unless you are willing to accept as theory some standard notions about the variable effects of such factors as social class and religion on behavior) Duberman cannot, given the character of her sample, engage in any meaningful assessment of the impact of one variable upon another.

Finally, as far as analysis is concerned Duberman is misleading in the extreme when she treats her respondents' subjective perceptions of the behavior and attitudes of others as though she were assessing those behaviors and attitudes themselves. This is particularly problematic in chapter 6 when relationships between stepchildren are assessed solely in terms of parental reports and in chapter 7 when outsiders' attitudes toward the families are assessed solely in terms of the perceptions of these attitudes which Duberman's respondents harbor.

The book exhibits other problems, not the least of which is Duberman's sloppy conceptual sense (e g., society—contrary to Duberman's reified usage—cannot devise formal behaviors for anything; she implies that it can [p. 3]), but it would be bad form to run up the score. Suffice it to note that after reading this book I know no more about the reconstituted family than before I read it. Unless you are asked to review it for one of the other journals purporting to keep sociologists informed about significant new works in the discipline, I suggest that you not squander your time reading this book.

Hunger as a Factor in Human Affairs. By Pitirim A. Sorokin. Translated and with a prologue by Elena P. Sorokin. Edited and with an introduction by T. Lynn Smith. Gainesville: University Press of Florida, 1975 Pp. xxxix+319. $12.50.

C. Peter Timmer

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Why should a book written during the now forgotten Russian famine of 1921–22 be published in 1975? Why should it be read? If it had been published in 1922, would it be read today? These are the obvious questions any reviewer must address when judging this book, especially in the light of the editor's introduction: "It is not too irresponsible to predict that for many years to come this book will be read and studied by people in many lands long after most of the current works in anthropology, economics, geography, political science, and sociology are almost completely forgotten" (p. xv).

A certain morbid fascination with the book is inevitable. It was written

by a starving and freezing scholar in the midst of a famine that he felt was caused by the revolution to which he was so hostile. As an exercise in intellectual history, the publishing of the book has merit. This is particularly true for the science of nutrition, because the knowledge of basic human nutrition and of the functional impact of under- and malnutrition demonstrated by Sorokin in 1921 must give pause to modern nutritional scientists concerned with generating the knowledge needed to help feed a hungry world.

The gross outlines of human nutrition were known by the time Sorokin was writing, and he was able to discuss the roles of energy, lipids, protein, and amino acids, and several of the vitamins and salts. The extraordinary aspect of the book, however, is his discussion of the functional impact of hunger and malnutrition. Hence the treatment of hunger and disease. "Particularly noticeable is the decrease in the number of white blood cells, which are protectors of the organism against bacteria (hence, during starvation, the decrease in immunity and rise of disease and of mortality)" (p. 59); of physical strength: "*The physical force of those in the well-fed strata of the population must be greater, on average, than of people of the same race, age, and sex in the strata that are ill fed*" (p. 60); of mental ability: "There is also a deformation *of memory and the ability to recall in general*" (p. 75); of the effects of eating meat: "It was noted that people, particularly primitive ones, who live predominantly on meat are rapacious, energetic, and aggressive. . . . Conversely, many vegetarian peoples and groups are mostly peaceful and not predatory" (p. 78); and of the impact on general productivity. "During these years [of the famine] the general productivity of labor decreased everywhere in the USSR, particularly in the centers where famine prevailed. This was true not only for the physical laborers, but also for the semi-intellectual occupations (printers, clerks, accountants, etc.). The attention, exactness, speed, energy, etc., once prevailing all disappeared" (p. 84).

It is small exaggeration to say that present nutritional science knows little more than did Sorokin, in a quantitative functional fashion, about any of these topics except the impact of meat on vigor. (The right balance of vegetable proteins can accomplish the same effect.) And it is precisely this type of functional information that is needed to justify programs aimed at improving nutritional status to economic planners allocating scarce resources in poor countries.

But it is not the treatment of these influences of hunger on the individual that raises the hope that the book will speak to the problems of today. This hope lies in the ultimate impact on society of widespread hunger. Sorokin is not troubled by the elements of sociobiology in his thesis. "A number of arrogant defenders of 'autonomy' in sociology and in some other disciplines may object to all the problems introduced here, and may consider the 'introduction' to be superfluous. My answer to this is that they are essential, as a starting point. The autonomy of biology does not preclude basing its disciplines on the facts of physics and chemistry, and biologists find this not to be shameful, but on the contrary, promote it to the

maximum. The same is true about the relationship between sociology and biology. By ignoring biology, the 'autonomists' are obliged to base their theories on fantasy. I am not a believer in such procedures" (p. 36).

Sorokin's belief in the primacy of biology must derive from his observations of a society on the bare edge of biological (nutritional) survival. Hunger and famine in such times must provide nearly the entire driving force of a society. "Hunger, or the threat of it, gives rise to war, when there are no other means of satisfying it, and war, in turn, gives rise to more hunger. These twins almost always are inseparable and they travel together all over the world" (p. 201). "When increasing starvation pinches the nutritional reflexes of a population, and when there are no other means to satisfy their hunger, starving people commit crime. . . . But when similar actions are committed by a mass of people, by a whole stratum of society . . . then it is called revolution, with the adjective 'social' " (p. 234). "As to the future, my thesis says: Other things being equal, if the satiety of the aggregates increases, statism and the role of the government in nutritional matters will decrease; contrarily, if adequacy of food supply decreases, and the danger of starvation increases (as was the case during the years of the war), the growth of the regulatory functions of the government in the economic-nutritional field will increase" (p. 301). And Sorokin's gloomy conclusion is that "nationalization, communization, and the development of statism leads to poverty, not to prosperity, and by no means do they improve the social conditions of the masses" (p. 319).

The difficulty, of course, is that neither Sorokin's casual historical empiricism nor subsequent history supports such a thesis and conclusion. The growth of statism in well-fed societies is undeniable, as is the improvement in the "social conditions of the masses" in Russia, China, and Cuba. The ultimate irony of the book is that Sorokin's own hunger must have been the major factor in his deterministic treatment of human affairs.

Planning and Organizing for Social Change. By Jack Rothman. New York: Columbia University Press, 1974. Pp. xvii+628. $20.00 (cloth); $7.50 (paper).

Richard A. Berk

University of California, Santa Barbara

If you are the kind of person who generally concurs with the reviews of movie critics and is satisfied with the summary form in which those assessments are typically presented, you may well find much to applaud in Jack Rothman's *Planning and Organizing for Social Change.* In essence, Professor Rothman responds to the very real need to transform scattered social science findings into brief, useful, substantive conclusions with direct implications for policy. In particular, he hopes to inform the efforts of day-to-day practitioners on the front lines of community social change.

The volume is perhaps best viewed as an encyclopedia of social science

findings abstracted largely from primary sources. Along with a team of reviewers, Rothman searched approximately 30 respected journals for the years 1964–70 for articles whose findings might assist community organizers, social workers, teachers, and the like. This "data base" was then informally supplemented with research from dissertations, papers read at professional meetings, and other sources. All told, nearly 1,000 such references were scrutinized and then organized into 10 broad categories, including "Political and Legislative Behavior," "Participation: Voluntary Associations and Primary Groups," and "Organizational Behavior Contextual Factors." These in turn were further broken down. Thus, under "Diffusion and Adoption of Innovations" were grouped such headings as "Target System Variables," "Attributes of the Innovator Proper," and "Attributes of Diffusion and Adoption Process." Finally, for each of these topics, the relevant research was summarized in the form of (1) generalizations, (2) an overall assessment of the degree to which the literature supported particular generalizations, and (3) a couple of paragraphs elaborating and qualifying resulting conclusions. For example, one generalization reads, "Professionals in general tend toward conventional, low-conflict political roles (+++)" (p. 93). The three plus signs indicate that the literature provided fairly strong support for the generalization.

The author readily acknowledges some of the flaws in his efforts, many of which seem an almost inevitable and thus easily forgiven consequence of any abstraction process: incomplete coverage of the literature, necessary brevity in exposition, etc. I note, for instance, that not a single economics journal is cited. Other difficulties may be briefly alluded to: superficial judgments on the methodological quality of reviewed research, problems in weighing studies by their coverage, quality and size of effects, and quality control over the review process itself. I am, for example, uneasy that no serious attempts at gauging interreviewer reliability are reported However, here too, given the world of finite resources in which we live, one can perhaps live with such flaws. On the other hand, recent social science controversy on such related problems as "urban flight," the efficacy of deterrence, and the impact of schooling on one's life chances should arouse genuine caution in even the most gullible reader.

There are also some difficulties in applying the generalizations even if taken at face value First, in order to apply the volume's advice, the concepts within each generalization must be operationalized. This is an enormous hurdle and one has only to sample a few of the generalizations to get a sense of the problems. For instance, "Some practitioners utilize a limited role set, others utilize an extended role set encompassing a range of subroles" (p. 61). While Rothman attempts to explain these concepts, it seems very hard to judge in practice what activities constitute distinct "roles."

Second, even the most rigorously phrased generalizations are at best ordinal statements: something is better or worse, larger or smaller, more effective or less effective. Moreover, the generalizations are typically stochastic predictions with qualifiers like "tends to," "usually," "some-

maximum. The same is true about the relationship between sociology and biology. By ignoring biology, the 'autonomists' are obliged to base their theories on fantasy. I am not a believer in such procedures" (p. 36).

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Second, even the most rigorously phrased generalizations are at best ordinal statements: something is better or worse, larger or smaller, more effective or less effective. Moreover, the generalizations are typically stochastic predictions with qualifiers like "tends to," "usually," "some-

times," etc. This is no doubt prudent and reflects the state of our knowledge. However, it means that the practitioner has little notion of the magnitude of the effects described, the chances that the generalization is substantially wrong in any given instance, or the likely size of the departure of the generalization from the true state of affairs. In more statistical terms, the practitioner has little idea of the magnitude of the regression coefficient, only a hint of its sampling distribution, and virtually no inkling of the dispersion around the predicted outcome.

It is, of course, ultimately up to practitioners to decide whether such information is helpful. In my experience, the primary benefits of such generalizations lie in the questions they pose about potential social change strategies and the suggestions they provide about the kinds of data that are needed to make more informed decisions. In short, they raise issues that might otherwise be ignored. While this is a very long way from the firm guidelines for action stated as the author's intent, the book may well make a nontrivial contribution.

Population Growth and Educational Planning in Developing Nations. By Gavin W. Jones. New York: Halsted Press, 1975 Pp. xiv+238. $14.95.

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University of Chicago

Gavin W. Jones, Senior Fellow in the Department of Demography at the Australian National University and former Population Council Representative in Indonesia, attempted to serve two purposes by writing this book. (1) to quantify the benefits to the educational systems of developing nations that would result from a reduction in birth rates, and (2) to encourage planners in ministries of education to pay greater attention to population policy matters in considering long-range educational development. The book serves the first purpose effectively but is less successful in achieving the second.

To accomplish his first purpose Jones depends heavily on three quite dissimilar case studies in which he employed different indices and methods of calculation, yielding uneven reports which are not directly comparable While 10 pages are devoted to Singapore, 33 are given to Thailand and 41 to Sri Lanka. Retrospective data are given for Singapore for the period 1957–71. Historical and projected data are given for Thailand, 1960–2000, and for Sri Lanka, 1963–98. Clearly the case studies were conducted as separate and independent ventures. They are brought together in this book for the purpose of illustrating quite different points.

The historical treatment of Singapore contains several somewhat surprising findings. Although the crude birthrate in Singapore declined from 42.7 in 1957 to 22 3 in 1971, the absolute expenditures on primary education more than doubled during this period. Although the birthrate had been declining since 1957, it was not until 1965 that universal primary education was achieved, and an additional four years passed before the

decrease in the number of primary school enrollments began. The costs of education in Singapore increased steadily between 1959 and 1973 because of rising wages for teachers and an expansion and diversification of secondary and higher education. Although primary education expenditures in 1959 were 62% of all educational expenditures, by 1973 the percentage had dropped to 40. The expansion and diversification of secondary and higher education had added greatly to the total costs while the apparent decrease in primary education costs due to declining fertility was more than matched by a corresponding increase in per pupil costs. Because of declining fertility it has been practical for Singapore to improve its provisions for secondary and higher education. The findings also illustrate the point that the costs of education are unlikely to decline even with a decrease in the number of primary pupils served because of a tendency for educational systems to apply the potential savings to expanding and diversifying the entire educational program.

The case study of Thailand shows that during the 1960s, despite considerable and rising expenditures on education, the bulk of the increase was used for what Jones calls "static expansion," that is, maintaining the enrollment rates and quality levels that existed at the beginning of the period. He explains the slow educational progress by saying, "Thailand has failed . . . to give sufficient emphasis to education to enable the coverage and quality of the system to improve rapidly in the face of very rapid population growth" (p. 117). He does not explain how the decision was made regarding the amount of emphasis education should be given.

The case study of Sri Lanka is used to show the estimated share of the gross national product that would be required under a variety of population growth rates and changes in the costs of educational inputs.

Although the title of this book indicates that it deals with educational planning, the reader will find it deficient in two respects. First, insofar as developing nations are concerned with education as a means of facilitating development, it is likely that adult literacy and vocational programs, which typically lie outside of the traditional educational establishment, are of major concern. Jones treats education as though it were only a synonym for schooling. Second, educational planning, or the decision-making and political activity of planners within ministries of education, is treated inadequately, if at all. Although the reader is told that Thailand "failed" to give sufficient emphasis to the coverage and quality of its educational system, the author gives no hint that he has any idea where the responsibility for this failure lies. Inasmuch as the author had worked as a demographic adviser to the National Economic Board in Thailand from 1969 to 1972, he might reasonably be expected to have an idea of whether the blame should be attributed to planners in the Ministry of Education or to other decision makers at higher levels in the government. Further, without a discussion of the alternative uses to which the Thai government put its resources it seems unduly harsh for the author to label the funding of "static expansion" as a failure to make significant progress toward other goals.

In the nine chapters which accompany the three case studies Jones successfully illustrates the point that the benefits to educational development which result from a reduction in birthrate are relatively long run, and he does demonstrate convincingly that a reduction in birthrate would facilitate the achievement of both quantitative and qualitative educational goals. Unfortunately he does not offer a useful critique of the planning activities of educational ministries, so the reader is left wondering just how Gavin Jones would employ population growth estimates practically in shaping educational planning in developing nations.

Drugs and Minority Oppression. By John Helmer. New York: Seabury Press, 1975. Pp. xi+192. $9.95.

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An awareness of social class is essential to understanding enforcement of drug laws in America because drug use in this country is and always has been chiefly a working-class phenomenon. This has been a specific cause, not a general consequence, of drug prohibition. Helmer's book examines several generations of Americans who have used a variety of drugs, all of which have at one time or another been considered to be narcotics. The primary focus of the study is on the classes of people who have used drugs rather than on individual drug use. The author seeks to explain public attitudes and social and legal policies in terms of an enduring struggle between economic classes.

Helmer explores what he calls the "mythology of narcotics" that has developed in the past century, how and why it has grown, and what purposes it has served, as well as its relationship to the truth about drugs themselves and about the people who use them. This mythology has played a major role in identifying the opposing sides and the measures that have been taken in the broader social conflict in which the drug issue has been enmeshed. Superficially it appears that labor markets, wage scales, and unemployment have little to do with drugs. In actuality, Helmer believes, the power and persistence of the drug mythology has derived from its extensive involvement with such matters, both as motivation and as rationalization for the methods that have been used to settle economic conflicts.

Helmer concludes that times of labor surplus, such as the late 1870s in California and the decade of the 1930s throughout the country, have been characterized by industrial unrest and working-class militancy, as well as heightened political conflict. Increased law enforcement, defined in terms of repression, has been the typical response of government to these manifestations of labor surplus. Drug enforcement is one way in which a basically repressive policy directed at an entire class of people can be implemented, under the pretext of checking some nefarious threat.

Helmer further concludes that a radical downturn in the business cycle,

the operation of a split labor market, and a deterioration of the expectations of all classes regarding their economic welfare are needed in order to make it probable that action on drugs will be taken. Public concern about drug use and the intensity of law enforcement efforts are basically functions of a surplus condition in the secondary labor market. Conversely, when unskilled labor has been scarce, popular anxiety about drug users among the unskilled labor force has been minimal.

The utilitarian argument that addiction leads to social harm is the key, Helmer believes, to understanding the punitiveness that has characterized American drug policy If the harm that results from drug addiction were suffered by the addict alone, incarceration would probably not be regarded as the proper response to addiction.

A few questionable assertions are made by Helmer. At one point he contends that "lethargy, flaccidity, laziness, passivity, inordinate sensuality—these are recurring images of the addict. The point here is that this stereotype was applied *only* to the working-class addict—and not only the addict, for it was and is still applied to the working-class individual in general" (p 16) The author overstates his case here. The above-mentioned characteristics have been attributed to drug users who are not members of the working class as well as to non-drug users of every social class. Elsewhere Helmer argues that it is fallacious to believe "the consumption of narcotics was widely diffused throughout American society, at least until 1920" (p 6), and that the pattern of working-class or lower-class narcotics use—which has been the acknowledged one since 1920, along with the related delinquency and crime required to finance drug purchases—has been the *effect* of the legislation. These contentions contradict the findings of virtually every historian who has examined 20th-century opiate dependency in the United States. The Harrison Act of 1914 was responsible to a considerable extent for changing the class, racial, and sexual characteristics of the drug-using population.

On the whole, however, Helmer's book is an informative study of a complex social problem that has occupied the attention of numerous individuals and government agencies. It helps to clarify the roots of the present drug policies which the federal government is pursuing and indicates why those policies cannot accomplish what their sponsors claim for them. The book should be of interest to sociologists, social and economic historians, drug-abuse specialists, and legislators, as well as to every individual who seeks to understand the origins of the present effort to curtail recreational drug use. Its sociological implications extend beyond the specific focus of drug-abuse control and have relevance to an understanding of other forms of class oppression in America.

Criminal Violence. By Lynn A. Curtis. Lexington, Mass.: D. C. Heath & Co., 1974. Pp. xxi+231. $15.00.

Violence, Race, and Culture. By Lynn A. Curtis. Lexington, Mass.: D. C. Heath & Co., 1975. Pp. xvii+168. $14.00.

Violence and Criminal Justice. Edited by Duncan Chappell and John Monahan. Lexington, Mass.: D. C. Heath & Co., 1975. Pp. xxiii+141. $13.00.

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Criminal Violence, by Lynn Curtis, developed out of the author's participation in the 1968–69 National Commission on the Causes and Prevention of Violence. It presents a greatly expanded and much more detailed version of the results of a 10% sample survey of police reports, involving homicide, assault, rape, and robbery in 17 large urban areas, upon which the Task Force on Individual Acts of Violence was partially based. The variables reported and themes discussed include the race, sex, and age relationships between offenders and victims; the nature of interpersonal relationships involved; offenders' motives; the extent of victim precipitation; the type of weapons used and the spatial distributions of criminal locations. The bulk of each chapter examines the basic data from the survey, comparing them with exhaustive summaries of findings culled from a wealth of previous U.S. and foreign studies. The apparent intention here is to relate to other data every single datum available rather than to integrate the material conceptually or theoretically. While the data coverage is encyclopedic, the studies Curtis has so voraciously consumed are regurgitated in a semidigested manner which is not at all appetizing to the reader. However, since the book is partly intended as a source of reference and compendium of "facts," the experience of a reviewer who reads it from cover to cover may be atypical.

Given the considerable criticism which greeted the national commission's report, it is disappointing to note that Curtis fails to confront the major theoretical and political objections raised by the critics. This is in direct contrast to a very careful consideration of technical problems of method. Despite scattered statements which relate violent crime to institutional racism and economic structure, the underlying conceptual and methodological assumptions adopted in *Criminal Violence* remain conservative and empiricist. That this may well reflect the book's origins in the Violence Commission's reports rather than the author's current intellectual position does not negate the applicability to *Criminal Violence* of the important criticisms of the Violence Commission's work.

The approach is empiricist: Curtis assumes that descriptions of facts are independent of, and can usefully precede, concept formation and theory construction. The approach is conservative: the crucial concepts, "violence" and "criminal," are derived not from any sociological paradigm

but from conventional definitions adopted by the FBI. These are the product of legal norms which, on the whole, reflect the value preferences of historically dominant groups. To equate criminal violence with offenses which appear in police records, while often accepted criminological procedure, is seriously misleading. The systematic exclusion of violence perpetrated by state agencies obviously distorts the profile of violence in urban America. Asking the police to provide the data on which sociological theory is to be constructed and allowing them to select the cases sampled only compounds the felony. It is not surprising, therefore, that the picture which emerges is one with a concentrated focus on crimes committed by inner-city blacks and a deafening silence on the use of violence by the state. This is important because the data presented in *Criminal Violence* provide the rationale for limiting discussion of the relationships between violence, race, and culture to an exclusive examination of violent crimes committed by blacks. An interest in black violence may be quite legitimate. What is questioned here is the portrayal of this specific type of violence as the dominant one in the United States

In the second volume reviewed here, *Violence, Race, and Culture,* Curtis offers a speculative essay which attempts to assess the validity of the subcultural explanation of violent crime by reviewing literature drawn from historical, structural, interactionist, and cultural perspectives The unified theory which emerges is documented with evidence derived from both empathic, participant studies of black culture and quantitative data from *Criminal Violence*. Curtis's central theoretical statement is that "racial-economic constraints have causal primacy in determining black behavior but that culture is a critical intervening variable" (p. 18) Here the failure to specify exactly what is meant by "critical intervening variable" is significant because the subsequent discussion of the nature of the relationships between economic structure, black culture, and violent crime might more accurately be interpreted as additive or interactive rather than intervening

The proposed theoretical model is comprehensive, complex, and eclectic. Interactional outcomes labeled as violent crime are more probable when one or both parties are integrated into a violent contraculture. While the reference group of the violent contraculture is primarily, though not exclusively, identified with poor, young, male, black residents of ghetto slums, its sources are located as much in the relationships with the dominant white society as in the internal cultural processes of the ghetto. A violent contraculture is created as each generation of young black males adapts to institutionalized racism and economic deprivation. It is reinforced by violent stimuli from the dominant culture and buttressed by processes of cultural transmission embodying the black experience of, and historical adaptations to, racial and economic oppression. For each generation, the sources of violent contraculture are at once historical, structural, and cultural.

Curtis rejects unidimensional views of black culture, especially those based on the "pejorative tradition" of black family pathology, social

disorganization, and cultural deprivation. Instead, he proposes the idea of a multidimensional cultural balance in which "some poor blacks simultaneously accept dominant cultural, black poverty subcultural, and violent contracultural values, behaviors and meanings" (p. 2). Violent outcomes are predicted where expressions of masculinity approved by the dominant culture, such as socioeconomic achievement, physical toughness, and sexual virility, are redirected and exaggerated within the violent contraculture. Emphasis is thus placed on the interdependence of the normative systems rather than viewing them as cultural isolates.

The eclecticism with which Curtis develops his argument, though a major strength of his work, is also a source of weakness. In the attempt to incorporate diverse viewpoints, the logical consistency of his unified theory is impaired. This is most apparent in conceptualizing the relationship between dominant culture and violent contraculture. Despite explicit adoption of Yinger's definition of contraculture, as involving values in conflict with the dominant ones, Curtis eventually comes to the view that the violent contraculture is essentially an exaggeration or involution of dominant values and that the difference is one of degree not kind (p. 46). This raises interesting questions which Curtis really fails to develop.

Curtis views dominant culture as influenced by corporate and political power elites. Among other things, dominant values involve normative support for the use of violence to preserve the status quo at home and U.S. hegemony abroad. This point logically could have led to an interesting comparison of the extent of support for the use of violence in the dominant culture as opposed to the black contraculture and the different purposes for which violence is legitimated in each. It might also have led to an examination of the extent to which any black violent contraculture is a direct response to violence by predominantly white agents of social control similarly imbued with exaggerated and involuted dominant values. However, Curtis's continued narrow focus on crimes committed by blacks leads him to rest content with the assumption that it is the black contraculture which is the more prone to violence. The interesting questions of the linkages between corporate and political power, the value patterns of dominant culture, and the legitimation and normative sanctioning of different types of violence are left unasked. The concentrated attention paid to violent crimes committed by blacks eventually serves to mystify the true nature of violence in the United States.

The collection entitled *Violence and Criminal Justice* merits less attention than do Curtis's books. Consisting of eight short papers originally presented at the 1973 Pacific Northwest Conference on Violence, it fails to avoid the common faults of such publications. The papers lack thematic continuity and are very uneven in quality. The editors do provide an introduction, but it is little more than a precis of the subsequent papers and fails to give any coherence to the volume. The general arguments contained in at least four of the papers—including those by Marvin Wolfgang who summarizes contemporary perspectives on violence, by Gilbert Geis and Herbert Edelhertz who review in turn the plight of the crime

victim and various administrative proposals for victim compensation, and by James Short who discusses the politicization of violent gangs—are well known through these authors' previous publications. Of the four remaining papers, two are interesting, not because of the arguments developed but as illustrative material on the underlife of the penal system. Seymour Halleck discusses the ethical and political questions involved in various forms of intervention to prevent violence. His reluctance to condemn biological interventions through psychosurgery, massive tranquilization, aversive conditioning, and castration (at least if the subject's informed consent is obtained) is all the more disturbing when coupled with John Monahan's report that all the current attempts to predict individuals' proneness to violence result in massive overprediction. Finally, Hans Toch's chatty presentation of the problems involved in attempting to reform the violent subculture which is developed in police departments and among prison guards is a useful antidote to Curtis's tendency to assume that violent subcultures are restricted to black ghettos.

Hacks, Blacks, and Cons. Race Relations in a Maximum Security Prison. By Leo Carroll. Lexington, Mass.: Lexington Books, 1974. Pp. v+259. $17.50.

Julian B. Roebuck

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Leo Carroll demonstrates that the structure of social relationships within prisons increasingly is taking on the character of race relations as a result of the concurrence of recent social changes of two kinds: (1) Humanitarian prison reform policies have weakened the power of prison guards, pitted custodians against prison administrators, undermined the convict solidarity of white inmates, and facilitated black solidarity among black inmates. (2) Outside racial-ethnic social movements have permeated prisons, enabling black prisoners to articulate an ideology of black nationalism with their prisoner status. This case study of one reform-oriented northeastern maximum-security state prison analyzes the nature and implication of these changes within one penal setting. Carroll conducted the study from September 1970 to September 1971, utilizing commendable overt, triangulated participant observation and interviewing techniques. His reconstruction of the setting appears incisive and creditable.

The power structure involved four conflicting groups: the administration, guards, cons (white inmates), and black inmates. An administration that was weak and vacillating in alleviating inmate deprivation, lessening regimentation, and facilitating inmate contacts with the outside eliminated the necessary rules, rituals, and routines by which inmate behavior had formerly been regulated. Due process of law procedures undermined the guards' work role. Leaders of inmate organizations were given wide latitudes of behavior and awesome negotiating power with the administration. More-

over, outside pressure groups were permitted entrance to counsel and socialize with members of these organizations without surveillance.

Justifiably fearing physical attacks from groups of black inmates should they enforce prison rules, and recriminations from superiors should they turn in disciplinary reports on inmate elites, the guards rebelled against the administration. Unable to secure compliance with minimum standards of conduct by the enforcement of a set of impersonal rules, they resorted to formal and illegal means to retain some semblance of order, including forming personal ties and engaging in illicit exchanges (e.g., prison goods) with inmates, overlooking serious disciplinary infractions, and forewarning inmates about impending disciplinary action and administrative policy, employing inmates in supervisory roles.

The cons were a congeries of conflictive cliques who feared and hated the blacks and were thoroughly manipulated and exploited by white elites. Mafia inmates, a clique of five to six syndicated criminals atop the white hierarchy, were recognized by *all* as prestigious, superior people. Performing an accommodating leadership role, they were called upon by the administration and by inmate groups to mediate and arbitrate conflicts between staff and inmates and among inmates. In crisis situations the administration utilized them as advisers and compromising emissaries. In return they received deference, freedom of movement, good jobs, and disciplinary immunity. At prison banquets attended by outside groups and dignitaries they sat at a corner table with tablecloth and silverware, and their food was brought to them and their guests while others, including the warden, had to stand in line. This coterie utilized the services of the wise guys (institutionalized prison toughs), the "politicians," and the keepers in maintaining a degree of prison stability necessary for their comfort and ease. It procured future outside jobs and union memberships for lesser elites. Virtually all blacks were members of the Afro-American Society, a semiautonomous organization led by a clique of six or seven black revolutionaries. This organization with the aid of outsiders had transformed the criminal mentality and identity of blacks into a revolutionary mentality enabling them to rationalize their crimes (even rape of black women) as the consequence of white oppression and colonialism. This society conducted and reported its own investigations of disciplinary measures taken against blacks, engineered black physical confrontations with guards, successfully threatened the administration with organized disturbances, and prevented blacks from discussing nonracial inmate problems with the administration.

The white inmates, guards, and administration lived in mortal fear of the black minority, who received lighter disciplinary measures than most whites. Despite a managed policy of racial integration a clear-cut pattern of racial avoidance and segregation (marked by reciprocal hatred) prevailed in everyday activities. Biracial interaction was limited to the achievement of mutual goals organized around drugs, sex, and contraband.

Carroll's findings demonstrate five important points: (1) Authority and power should by all means reside with the keepers rather than with the

inmates. (2) Managed racial integration does not work in a prison setting. (3) It is risky to grant outsiders entrance to politicize prisoners. (4) It is poor policy to support racial-political inmate groups without administrative surveillance. In this vein I suggest that we might make provisions for political offenders in our criminal codes and incarcerate them in separate institutions when necessary. We must not support inmate indoctrination programs that convince rapists and burglars that they are criminally innocent political prisoners. Perhaps inmates could be taught that class differentials rather than racial differences are at the root of many of their difficulties (5) The mafia exists in and out of prison. This is probably the most important finding.

Carroll unfortunately invokes too many sociological theories and concepts in support of his tenable findings. Nevertheless, having worked in several reform-oriented penal settings, I recommend this book to criminologists and penologists as an excellent manual on how *not* to plan, staff, and run correctional institutions. Scholars and students in the field of race relations as well as civil rights advocates should find the volume challenging and informative. Carroll clearly demonstrates that some racial programs initiated on behalf of a racial minority may result in the unintended dominance of that minority—and the consequent subordination of the former superordinate group.

The Dangerous Class: Crime and Poverty in Columbus, Ohio, 1860–1885. By Eric H. Monkkonen. Cambridge, Mass.: Harvard University Press, 1975. Pp. xii+186. $13.50.

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University of Cincinnati

This study by Eric Monkkonen is of special interest to criminologists and sociologists who are working (or supervising graduate students) on problems of urban crime and poverty. The book is a presentation of statistical research and is therefore better studied than read. The transition from dissertation to book is not complete, but the reader who carefully works his or her way through the document will be able to examine the basic problems of historical research on criminality, that is, the pitfalls and compromises involved in using existing agency records and modern analytical methodologies to answer historical questions.

Monkkonen wants to draw inferences about the relationship of crime and poverty to urbanization and industrialization without relying upon the conscious explanation given at the time by the dominant class. His field of research thus becomes the aggregate socioeconomic data that can be obtained from court and poorhouse records, among others, which may of course, have a bias similar to that of the dominant class. It is educational to think through with the author the many problems he faced and the assumptions he was forced to make because there is so little information available about the life of the "dangerous class."

However, by quantifying such "objective" material, Monkkonen presents an interesting examination of two constructs—urbanization and industrialization—which tend to be used to explain the undesirable aspects of living in urban America. These constructs can be convenient devices for answering historical and social questions without having to confront the issues of who is and is not served by a particular social structure. Today, we can see the intellectual weaknesses in Wolfgang's theory of the subculture of violence, whereas yesterday we used this theory as an explanatory mechanism with regard to such enterprises as the "war on poverty" and the urban renewal movement. Similarly, Monkkonen questions those explanations which draw upon theories of urbanization and industrialization to account for changing crime patterns. That this single piece of research is flawed is less important than the evidence that the self-critical nature of scholarship seems to be removing a deceptive veil, albeit after the constructs have served their purposes. One of the most rewarding aspects of history as a discipline is this capacity to teach us how empty our conventional wisdoms often are.

The study is an attempt to define and objectify the hodge-podge of state and local records available—court records, census data, city directories, economic statistics—about Columbus during the period from 1860 to 1885. The reader who takes the time to follow carefully each step in what the author admits is a tortuous route will be rewarded. Harvard University Press is to be commended for bringing this study to a wider audience, but it has been lax in its choice of a format, especially in decisions concerning the 54 tables, 12 figures, and the footnotes, which provide so much of the substance of the argument. Footnotes would have been more helpful to the reader on the page with the text, and the visual displays should certainly have been less cryptically labeled and irksomely placed. An introduction by a senior scholar, to frame the ideas, and a bibliographical essay on the issues involved and the city of Columbus would have been welcome.

One of the major strengths of the book is the visibility of the thought processes of the researcher. For example, if we ask how to define "urbanization" we can see the necessary leaps in logic, compromises on what may serve as acceptable data, and available manipulations for separating the noise from the message. Monkkonen tells us how, and often why, he does what he does in approaching the data. He concludes that the assumed causal relationship between urbanization/industrialization and increases in poverty and crime cannot be supported. Indeed, when logic is not distorted by ideology it seems to lead to the opposite conclusion. There were changes in the nature of crimes committed in Columbus during the period studied, but the extent of poverty and crime cannot be uncritically linked to the city's growth process. More particular causes for variations in these social problems must be sought.

The essential weakness of the study, however, stems from the problems inherent in the research materials that were used. Richard Quinney has summarized this difficulty: "For us, then, the meaning of criminal statis-

tics is clear: they represent the nature and extent of crime recognized in the society at a particular time. . . . In the final analysis, crime rates have to be understood as *political* devices" (*Criminology: Analysis and Critique of Crime in America* [Boston: Little, Brown & Co., 1975], p. 23). The numbers of crimes uncovered in official records reflect many factors, ranging from the size of the police force to the fears and attitudes of the municipal elite. Court records are probably a better indicator of these factors than of the quantity of "real events" occurring in the city that fit a certain definition of criminal behavior. Although this is a limitation which all research of this type must confront, it is to be hoped that we will see further inquiries into 19th-century crime and the growth of cities and industrialism.

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Format of References: List all items alphabetically by author (providing the full list of multiple authors) and, within author(s), by year of publication, in an appendix titled "References." Examples follow:

Davis, K 1963*a* "Social Demography " Pp 124–37 in *The Behavioral Sciences Today,* edited by Bernard Berelson New York Basic

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The Population Ecology of Organizations[1]

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A population ecology perspective on organization-environment relations is proposed as an alternative to the dominant adaptation perspective. The strength of inertial pressures on organizational structure suggests the application of models that depend on competition and selection in populations of organizations. Several such models as well as issues that arise in attempts to apply them to the organization-environment problem are discussed.

I INTRODUCTION

Analysis of the effects of environment on organizational structure has moved to a central place in organizations theory and research in recent years. This shift has opened a number of exciting possibilities. As yet nothing like the full promise of the shift has been realized. We believe that the lack of development is due in part to a failure to bring ecological models to bear on questions that are preeminently ecological. We argue for a reformulation of the problem in population ecology terms.

Although there is a wide variety of ecological perspectives, they all focus on selection. That is, they attribute patterns in nature to the action of selection processes. The bulk of the literature on organizations subscribes to a different view, which we call the adaptation pespective.[2] According

1 This research was supported in part by grants from the National Science Foundation (GS-32065) and the Spencer Foundation Helpful comments were provided by Amos Hawley, François Nielsen, John Meyer, Marshall Meyer, Jeffrey Pfeffer, and Howard Aldrich.

2 There is a subtle relationship between selection and adaptation. Adaptive learning for individuals usually consists of selection among behavioral responses. Adaptation for a population involves selection among types of members More generally, processes involving selection can usually be recast at a higher level of analysis as adaptation processes. However, once the unit of analysis is chosen there is no ambiguity in distinguishing selection from adaptation. Organizations often adapt to environmental conditions in concert and this suggests a systems effect. Though few theorists would deny the existence of such systems effects, most do not make them a subject of central concern It is important to notice that, from the point of view embraced by sociologists whose interests focus on the broader social system, selection in favor of organizations with one set of properties to the disfavor of those with others is often an adaptive process. Societies and communities which consist in part of formal organizations adapt partly through processes that adjust the mixture of

to the adaptation perspective, subunits of the organization, usually managers or dominant coalitions, scan the relevant environment for opportunities and threats, formulate strategic responses, and adjust organizational structure appropriately.

The adaptation perspective is seen most clearly in the literature on management. Contributors to it usually assume a hierarchy of authority and control that locates decisions concerning the organiation as a whole at the top. It follows, then, that organizations are affected by their environments according to the ways in which managers or leaders formulate strategies, make decisions, and implement them. Particularly successful managers are able either to buffer their organizations from environmental disturbances or to arrange smooth adjustments that require minimal disruption of organizational structure.

A similar perspective, often worded differently, dominates the sociological literature on the subject. It plays a central role in Parsons's (1956) functional analysis of organization-environment relations and it is found in the more strictly Weberian tradition (see Selznick 1957). It is interesting to note that, while functionalists have been interested in system effects and have based much of the logic of their approach on survival imperatives, they have not dealt with selection phenomena. This is probably a reaction against organization theory which reflects social Darwinism.

Exchange theorists have also embraced the adaptation perspective (Levine and White 1961). And it is natural that theories emphasizing decision making take the adaptation view (March and Simon 1958; Cyert and March 1963). Even Thompson's (1967) celebrated marriage of open-systems and closed-systems thinking embraced the adaptation perspective explicitly (see particularly the second half of Thompson's book).

Clearly, leaders of organizations do formulate strategies and organizations do adapt to environmental contingencies. As a result at least some of the relationship between structure and environment must reflect adaptive behavior or learning. But there is no reason to presume that the great structural variability among organizations reflects only or even primarily adaptation.

There are a number of obvious limitations on the ability of organizations to adapt. That is, there are a number of processes that generate structural inertia. The stronger the pressures, the lower the organizations'

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various kinds of organizations found within them Whereas a complete theory of organization and environment would have to consider both adaptation and selection, recognizing that they are complementary processes, our purpose here is to show what can be learned from studying selection alone (see Aldrich and Pfeffer [1976] for a synthetic review of the literature focusing on these different perspectives).

adaptive flexibility and the more likely that the logic of environmental selection is appropriate. As a consequence, the issue of structural inertia is central to the choice between adaptation and selection models.

The possibility that organization structure contains a large inertial component was suggested by Burns and Stalker (1961) and Stinchcombe (1965). But, on the whole the subject has been ignored. A number of relevant propositions can be found in the organizations literature, however.

Inertial pressures arise from both internal structural arrangements and environmental constraints. A minimal list of the constraints arising from internal considerations follows.

1. An organization's investment in plant, equipment, and specialized personnel constitutes assets that are not easily transferable to other tasks or functions. The ways in which such sunk costs constrain adaptation options are so obvious that they need not be discussed further.

2. Organizational decision makers also face constraints on the information they receive. Much of what we know about the flow of information through organizational structures tells us that leaders do not obtain anything close to full information on activities within the organization and environmental contingencies facing the subunits.

3. Internal political constraints are even more important. When organizations alter structure, political equilibria are disturbed. As long as the pool of resources is fixed, structural change almost always involves redistribution of resources across subunits. Such redistribution upsets the prevailing system of exchange among subunits (or subunit leaders). So at least some subunits are likely to resist any proposed reorganization. Moreover, the benefits of structural reorganization are likely to be both generalized (designed to benefit the organization as a whole) and long-run. Any negative political response will tend to generate short-run costs that are high enough that organizational leaders will forego the planned reorganization. (For a more extensive discussion of the ways in which the internal political economy of organizations impedes change or adaptation, see Downs [1967] and Zald [1970].)

4. Finally, organizations face constraints generated by their own history. Once standards of procedure and the allocation of tasks and authority have become the subject of normative agreement, the costs of change are greatly increased. Normative agreements constrain adaptation in at least two ways. First, they provide a justification and an organizing principle for those elements that wish to resist reorganization (i.e., they can resist in terms of a shared principle). Second, normative agreements preclude the serious consideration of many alternative responses. For example, few research-oriented universities seriously consider adapting to

declining enrollments by eliminating the teaching function. To entertain this option would be to challenge central organizational norms.[8]

The external pressures toward inertia seem to be at least as strong. They include at least the following factors.

1. Legal and fiscal barriers to entry and exit from markets (broadly defined) are numerous. Discussions of organizational behavior typically emphasize barriers to entry (state licensed monopoly positions, etc.). Barriers to exit are equally interesting. There are an increasing number of instances in which political decisions prevent firms from abandoning certain activities. All such constraints on entry and exit limit the breadth of adaptation possibilities.

2. Internal constraints upon the availability of information are paralleled by external constraints. The acquisition of information about relevant environments is costly particularly in turbulent situations where the information is most essential. In addition, the type of specialists employed by the organization constrains both the nature of the information it is likely to obtain (see Granovetter 1973) and the kind of specialized information it can process and utilize.

3. Legitimacy constraints also emanate from the environment. Any legitimacy an organization has been able to generate constitutes an asset in manipulating the environment. To the extent that adaptation (e.g., eliminating undergraduate instruction in public universities) violates the legitimacy claims, it incurs considerable costs. So external legitimacy considerations also tend to limit adaptation.

4. Finally, there is the collective rationality problem. One of the most difficult issues in contemporary economics concerns general equilibria. If one can find an optimal strategy for some individual buyer or seller in a competitive market, it does not necessarily follow that there is a general equilibrium once all players start trading. More generally, it is difficult to establish that a strategy that is rational for a single decision maker will be rational if adopted by a large number of decision makers. A number of solutions to this problem have been proposed in competitive market theory, but we know of no treatment of the problem for organizations generally. Until such a treatment is established we should not presume that a course of action that is adaptive for a single organization facing some changing environment will be adaptive for many competing organizations adopting a similar strategy.

A number of these inertial pressures can be accommodated within the adaptation framework. That is, one can modify and limit the perspective in order to consider choices within the constrained set of alternatives. But

[8] Meyer's (1970) discussion of an organization's charter adds further support to the argument that normative agreements arrived at early in an organization's history constrain greatly the organization's range of adaptation to environmental constraints.

to do so greatly limits the scope of one's investigation. We argue that in order to deal with the various inertial pressures the adaptation perspective must be supplemented with a selection orientation.

We consider first two broad issues that are preliminary to ecological modeling. The first concerns appropriate units of analysis. Typical analyses of the relation of organizations to environments take the point of view of a single organization facing an environment. We argue for an explicit focus on populations of organizations. The second broad issue concerns the applicability of population ecology models to the study of human social organization. Our substantive proposal begins with Hawley's (1950, 1968) classic statement on human ecology. We seek to extend Hawley's work in two ways: by using explicit competition models to specify the process producing isomorphism between organizational structure and environmental demands, and by using niche theory to extend the problem to dynamic environments. We argue that Hawley's perspective, modified and extended in these ways, serves as a useful starting point for population ecology theories of organizations.

II. POPULATION THINKING IN THE STUDY OF ORGANIZATION-ENVIRONMENT RELATIONS

Little attention is paid in the organizations literature to issues concerning proper units of analysis (Freeman 1975). In fact, choice of unit is treated so casually as to suggest that it is not an issue. We suspect that the opposite is true—that the choice of unit involves subtle issues and has far-reaching consequences for research activity. For instance, in the case at hand, it determines which of several ecological literatures can be brought to bear on the study of organization-environment relations.

The comparison of unit choice facing the organizational analyst with that facing the bioecologist is instructive. To oversimplify somewhat, ecological analysis is conducted at three levels: individual, population, and community. Events at one level almost always have consequences at other levels. Despite this interdependence, population events cannot be reduced to individual events (since individuals do not reflect the full genetic variability of the population) and community events cannot be simply reduced to population events. Both the latter employ a population perspective which is not appropriate at the individual level.

The situation faced by the organizations analyst is more complex. Instead of three levels of analysis, he faces at least five: (1) members, (2) subunits, (3) individual organizations, (4) populations of organizations, and (5) communities of (populations of) organizations. Levels 3–5 can be seen as corresponding to the three levels discussed for general ecology, with the individual organization taking the place of the individual organ-

ism. The added complexity arises because organizations are more nearly decomposable into constituent parts than are organisms. Individual members and subunits may move from organization to organization in a manner which has no parallel in nonhuman organization.

Instances of theory and research dealing with the effects of environments on organizations are found at all five levels. For example, Crozier's well-known analysis of the effects of culture on bureaucracy focuses on the cultural materials members bring to organizations (1964). At the other end of the continuum we find analyses of "organizational fields" (Turk 1970; Aldrich and Reiss 1976). But, the most common focus is on *the* organization and *its* environment. In fact, this choice is so widespread that there appears to be a tacit understanding that individual organizations are the appropriate units for the study of organization-environment relations.

We argue for a parallel development of theory and research at the population (and, ultimately, the community) level. Because of the differing opinions about levels of analysis, "population" has at least two referents. Conventional treatments of human ecology suggest that the populations relevant to the study of organization-environment relations are those aggregates of members attached to the organization or, perhaps, served by the organization. In this sense, the organization is viewed as analogue to a community: it has collective means of adapting to environmental situations. The unit character of a population so defined depends on shared fate. All members share to some extent in the consequences of organizational success or failure.

We use the term population in a second sense: to refer to aggregates of organizations rather than members. Populations of organizations must be alike in some respect, that is, they must have some unit character. Unfortunately, identifying a population of organizations is no simple matter. The ecological approach suggests that one focus on common fate with respect to environmental variations. Since all organizations are distinctive, no two are affected identically by any given exogenous shock. Nevertheless, we can identify classes of organizations which are relatively homogeneous in terms of environmental vulnerability. Notice that the populations of interest may change somewhat from investigation to investigation depending on the analyst's concern. Populations of organizations referred to are not immutable objects in nature but are abstractions useful for theoretical purposes.

If we are to follow the lead of population biologists, we must identify an analogue to the biologist's notion of species. Various species are defined ultimately in terms of genetic structure. As Monod (1971) indicates, it is useful to think of the genetic content of any species as a blueprint. The blueprint contains the rules for transforming energy into structure.

Consequently all of the adaptive capacity of a species is summarized in the blueprint. If we are to identify a species analogue for organizations, we must search for such blueprints. These will consist of rules or procedures for obtaining and acting upon inputs in order to produce an organizational product or response.

The type of blueprint one identifies depends on substantive concerns. For example, Marschak and Radner (1972) employ the term "organizational form"[4] to characterize the key elements of the blueprint as seen within a decision-making framework. For them the blueprint or form has two functions: an information function that describes the rules used in obtaining, processing, and transmitting information about the states of external environments, and an activity function that states the rules used in acting on received information so as to produce an organizational response. To the extent that one can identify classes of organizations that differ with regard to these two functions, one can establish classes or forms of organization.

Since our concerns extend beyond decision making, however, we find Marschak and Radner's definition of forms too limiting. In fact, there is no reason to limit a priori the variety of rules or functions that may define relevant blueprints. So for us, an organizational form is a blueprint for organizational action, for transforming inputs into outputs. The blueprint can usually be inferred, albeit in somewhat different ways, by examining any of the following: (1) the formal structure of the organization in the narrow sense—tables of organization, written rules of operation, etc.; (2) the patterns of activity within the organization—what actually gets done by whom; or (3) the normative order—the ways of organizing that are defined as right and proper by both members and relevant sectors of the environment.

To complete the species analogue, we must search for qualitative differences among forms. It seems most likely that we will find such differences in the first and third areas listed above, formal structure and normative order. The latter offers particularly intriguing possibilities. Whenever the history of an organization, its politics, and its social structure are encoded in a normative claim (e.g., professionalization and collegial authority), one can use these claims to identify forms and define populations for research.

Having defined the organizational form, we can provide a more precise definition of a population of organizations. Just as the organizational analyst must choose a unit of analysis, so must he choose a system for study. Systems relevant to the study of organization-environment relations are

[4] The term "organizational form" is used widely in the sociological literature (see Stinchcombe 1965).

usually defined by geography, by political boundaries, by market or product considerations, etc. Given a systems definition, a population of organizations consists of all the organizations within a particular boundary that have a common form. That is, the population is the form as it exists or is realized within a specified system.

Both uses of the term population (and the ecological theories implied thereby) are likely to prove beneficial to the study of organizational structure. The first, more common, view suggests that organizational structure ought to be viewed as an outcome of a collective adaptive process. According to this view, structure and change ought to depend on the adaptiveness of subunits and on the differential access of subunits to environmental resources. The second view ignores the adaptive activities of elements within the organization except as they constitute organizational structure. It focuses on the organization as an adapting unit. Certainly both perspectives are needed. We are concerned here only with the latter, however.

Finally, we would like to identify the properties of populations most interesting to population ecologists. The main concern in this regard was expressed clearly by Elton (1927): "In solving ecological problems we are concerned with *what animals do* in their capacity as whole, living animals, not as dead animals or as a series of parts of animals. We have next to study the circumstances under which they do those things, and, most important of all, the limiting factors which prevent them from doing certain other things. By solving these questions it is possible to discover the reasons for *the distribution and numbers of animals in nature*." Hutchinson (1959) in the subtitle to his famous essay, "Homage to Santa Rosalia," expressed the main focus even more succinctly: "Why Are There So Many Kinds of Animals?" Taking our lead from these distinguished ecologists, we suggest that a population ecology of organizations must seek to understand the distributions of organizations across environmental conditions and the limitations on organizational structures in different environments, and more generally seek to answer the question, Why are there so many kinds of organizations?

III. DISCONTINUITIES IN ECOLOGICAL ANALYSIS

Utilization of models from ecology in the study of organizations poses a number of analytic challenges involving differences between human and nonhuman organizations with regard to their essential ingredients. Consider, first, the nongenetic transmission of information. Biological analyses are greatly simplified by the fact that most useful information concerning adaptation to the environment (which information we call structure) is transmitted genetically. Genetic processes are so nearly invariant that

extreme continuity in structure is the rule. The small number of imperfections generates structural changes, which, if accepted by the environment, will be transmitted with near invariance. The extreme structural invariance of species greatly simplifies the problem of delimiting and identifying populations. More important, the adaptiveness of structure can be unambiguously identified with net reproduction rates. When a population with given properties increases its net reproduction rate following an environmental change, it follows that it is being selected for. This is why modern biologists have narrowed the definition of fitness to the net reproductive rate of population.

Human social organization presumably reflects a greater degree of learning or adaptation. As a result it is more difficult to define fitness in a precise way. Under at least some conditions, organizations may undergo such extreme structural change that they shift from one form to another. As a result, extreme adaptation may give rise to observed changes that mimic selection. This is particularly problematic when the various organizational forms are similar on many dimensions.

We have argued previously (Hannan and Freeman 1974) for a composite measure of fitness that includes both selection (actual loss of organizations) and mobility among forms (extreme adaptation). Fitness would then be defined as the probability that a given form of organization would persist in a certain environment We continue to believe that such an approach has value, but we now believe that it is premature to combine adaptation and selection processes. The first order of business is to study selection processes for those situations in which inertial pressures are sufficiently strong that mobility among forms is unlikely.

Furthermore, it is worth noting that the capacity to adapt is itself subject to evolution (i.e., to systematic selection). As we argue below, organizations develop the capacity to adapt at the cost of lowered performance levels in stable environments. Whether or not such adaptable organizational forms will survive (i.e., resist selection) depends on the nature of the environment and the competitive situation. Therefore, a selection point of view treats high levels of adaptability as particular evolutionary outcomes.

There is a second sense in which human ecology appears to differ from bioecology. Blau and Scott (1962) point out that, unlike the usual biological situation, individual organizations (and populations of organizations) have the potential to expand almost without limit. The expandability of primitive elements is a problem because of our focus on the distribution of organizational forms over environments. A given form (e.g., formal bureaucracy) can expand throughout some system, market, or activity, either because one bureaucracy grows or because many bureaucracies are founded. Either process will generate an increase in the prev-

alence of bureaucratic organizational activity. A literal application of population ecology theory to the problem of organizational change would involve simply counting relative numbers in populations. Such a procedure may miss a phenomenon of central interest to the organizational analyst. Winter (1964), in discussing the analytic problem raised here, suggests distinguishing between survival, which describes the fate of individual organizations, and viability, which describes the "share of market" of a given organizational form.

We find at least as much merit in another perspective on the issue of size. Many theorists have asserted that structural change attends growth; in other words, a single organization cannot grow indefinitely and still maintain its original form. For instance, a mouse could not possibly maintain the same proportion of body weight to skeletal structure while growing as big as a house. It would neither look like a mouse nor operate physiologically like a mouse. Boulding (1953) and Haire (1959) argue that the same is true for organizations. Caplow (1957), building on work by Graicunas (1933) and others, argues that the ability of each member of an organization to carry on face-to-face interactions with each of the others declines with the number of organizational participants. This creates a shift in the nature of interactions such that they assume a more impersonal, formal style. Blau and a number of coauthors have argued for similar causal effects of size on structure (Blau and Scott 1962, pp. 223–42; Blau and Schoenherr 1971; Blau 1972). If it is true that organizational form changes with size, selection mechanisms may indeed operate with regard to the size distribution. When big organizations prevail it may be useful to view this as a special case of selection, in which the movement from "small form" to "large form" is theoretically indistinguishable from the dissolution ("death") of small organizations and their replacement by (the "birth" of) large organizations.

In sum, we have identified a number of challenges. The first concerns the two sources of change, selection and adaptive learning. We feel that the organizations literature has overemphasized the latter at the expense of the former. Much more is known about decision-making practices, forecasting, and the like than about selection in populations of organizations. The second challenge involves the distinction between selection and viability. Whether such a distinction is necessary depends on the results of research on size which is currently being pursued by many organization researchers.

IV. THE PRINCIPLE OF ISOMORPHISM

In the best developed statement of the principles of human ecology, Hawley (1968) answers the question of why there are so many kinds of

organizations. According to Hawley, the diversity of organizational forms is isomorphic to the diversity of environments. In each distinguishable environmental configuration one finds, in equilibrium, only that organizational form optimally adapted to the demands of the environment. Each unit experiences constraints which force it to resemble other units with the same set of constraints. Hawley's explanation places heavy emphasis on communication patterns and structural complements of those patterns: "[organization units] must submit to standard terms of communication and to standard procedures in consequence of which they develop similar internal arrangements within limits imposed by their respective sizes" (1968, p. 334).

While the proposition seems completely sound from an ecological perspective, it does not address a number of interesting considerations. There are at least two respects in which the isomorphism formulation must be modified and extended if it is to provide satisfactory answers to the question posed The first modification concerns the mechanism or mechanisms responsible for equilibrium. In this respect, the principle of isomorphism must be supplemented by a criterion of selection and a competition theory. The second modification deals with the fact that the principle of isomorphism neither speaks to issues of optimum adaptation to changing environments nor recognizes that populations of organizations often face multiple environments which impose somewhat inconsistent demands. An understanding of the constraints on organizational forms seems to require modeling of multiple, dynamic environments. Of course, we cannot fully extend Hawley's principle here. We attempt only to outline the main issues and suggest particular extensions.

V. COMPETITION THEORY

The first of the needed extensions is a specification of the optimization process responsible for isomorphism. We have already discussed two mechanisms: selection and adaptive learning. Isomorphism can result either because nonoptimal forms are selected out of a community of organizations or because organizational decision makers learn optimal responses and adjust organizational behavior accordingly. We continue to focus on the first of these processes: selection.

Consideration of optimization raises two issues: Who is optimizing, and what is being optimized? It is quite commonly held, as in the theory of the firm, that organizational decision makers optimize profit over sets of organizational actions. From a population ecology perspective, it is the environment which optimizes.[5] Whether or not individual organiza-

[5] In biological applications, one assumes that power (in the physical sense) is optimized by natural selection in accordance with the so-called Darwin-Lotka law.

tions are consicously adapting, the environment selects out optimal combinations of organizations. So if there is a rationality involved, it is the "rationality" of natural selection. Organizational rationality and environmental rationality may coincide in the instance of firms in competitive markets. In this case, the optimal behavior of each firm is to maximize profit and the rule used by the environment (market, in this case) is to select out profit maximizers. Friedman (1953) makes use of this observation to propose a justification of the theory of the firm in terms of the principles of evolution. However, Winter (1964) has argued convincingly that the actual situation is much more complicated than this and that it is most unusual for individual rationality and environmental or market rationality to lead to the same optima. When the two rationalities do not agree, we are concerned with the optimizing behavior of the environment.

A focus on selection invites an emphasis on competition. Organizational forms presumably fail to flourish in certain environmental circumstances because other forms successfully compete with them for essential resources. As long as the resources which sustain organizations are finite and populations have unlimited capacity to expand, competition must ensue.

Hawley (1950, pp. 201–3) following Durkheim (1947) among others, places a heavy emphasis on competition as a determinant of patterns of social organization. The distinctive feature of his model is the emphasis on the indirect nature of the process: "The action of all on the common supply gives rise to a reciprocal relation between each unit and all the others, if only from the fact that what one gets reduces by that amount what the others can obtain . . . without this element of indirection, that is, unless units affect one another through affecting a common limited supply, competition does not exist" (Hawley 1950, p. 202). In Hawley's model, competition processes typically involve four stages: (1) demand for resources exceeds supply; (2) competitors become more similar as standard conditions of competition bring forth a uniform response; (3) selection eliminates the weakest competitors; and (4) deposed competitors differentiate either territorially or functionally, yielding a more complex division of labor.

It is surprising that there is almost no reliance on competitive mechanisms in Hawley's later work. In particular, as we noted above, the rationale given for the isomorphism principle uses an adaptation logic. We propose to balance that treatment by adding an explicit focus on competition as a mechanism producing isomorphism.[6] In so doing, we can bring a rich set of formal models to bear on the problem.

For the case of human social organization, one might argue that selection optimizes the utilization of a specific set of resources including but not restricted to the power and the time of members.

[6] We include only the first and third of Hawley's stages in our model of competi-

The first step in constructing an ecological model of competition is to state the nature of the population growth process. At a minimum we wish the model to incorporate the idea that resources available at any moment for each form of organization are finite and fixed. This corresponds with Hawley's notion of limited supply and Stinchcombe's (1965) argument that human communities have limited "capacities for organizing." We also wish to incorporate the view that the rate at which units are added to populations of organizations depends on how much of the fixed capacity has already been exhausted. The greater the unexhausted capacity in an environment, the faster should be the rate of growth of populations of organizations. But the rate at which populations of organizations can expand into unused capacity varies among forms of organization. So there are two distinctive ecological considerations: the capacity of the environment to support forms of organization and the rate at which the populations grow (or decline) when the environmental support changes.

In order to state the model formally, it is helpful to begin with the control function that Hummon, Doreian, and Teuter (1975) use to add dynamic considerations to Blau's theory of size and differentiation. The control model states that the rate of change in the size of any unit (here a population of organizations) varies proportionately with the difference between existing size, X, and the equilibrium level of size, X^*, permitted in that environment. Then one possible representation would be

$$\frac{dX}{dt} = f(X^* - X) = r(X^* - X). \tag{1}$$

In (1) X^* and r represent the limited supply or environmental capacity and the structural ability of the population of organizations to respond to changes in the environment, respectively.

A particular form of the general growth model in (1) underlies most population ecology work on competition. This is the logistic growth model (for per capita growth):

$$\frac{dX_1}{dt} = r_1 X_1 \left(\frac{k_1 - X_1}{k_1}\right) \tag{2}$$

where X_1 denotes population size, k_1 is the capacity of the environment to support X_1 (this parameter is usually called the carrying capacity), and r_1 is the so-called natural rate of increase of the population or the rate at which the population grows when it is far below the carrying capacity.

As we indicated above, both k and r are ecological parameters of funda-

tion. We prefer to treat uniformity of response and community diversity as consequences of combinations of certain competitive processes and environmental features

mental importance. Our research group has begun to compare various forms of organization by estimating the parameters of models like (2) for each form of organization. We have been successful to date in relating structural features of organizations such as complexity of core activity to variations in r and k (Nielsen and Hannan 1977; Freeman and Brittain 1977). This work, together with that of Hummon et al. (1975), gives us confidence that the model in (1) and/or (2) gives a good approximation of the growth of populations of organizations.

Up to this point we have presumed that the limits on growth reflect the finite nature of the environment (e.g., community wealth and mix of occupational skills). It is now time to reintroduce competition. According to Hawley, competition enters indirectly when the competitors lower the fixed supply. We can model this by following the lead of bioecologists and extending the logistic growth model. For example, consider a second population of organizations whose size is denoted by X_2. The two populations are said to compete if the addition of units of either decreases the rate of growth of the other. This will be the case when both populations are sustained by the same types of resources. Then the appropriate model is represented in the following system of growth equations (known as the Lotka-Volterra equations for competing populations):

$$
\begin{aligned}
\frac{dX_1}{dt} &= r_1 X_1 \left(\frac{k_1 - X_1 - \alpha_{12} X_2}{k_1} \right) \\
\frac{dX_2}{dt} &= r_2 X_2 \left(\frac{k_2 - X_2 - \alpha_{21} X_1}{k_2} \right).
\end{aligned}
\tag{3}
$$

The coefficients α_{12} and α_{21}, called competition coefficients, denote the magnitude of the effect of increases in one population on the growth of the other. In this simple formulation, the only consequence of competition is to lower the carrying capacity of the environment for a population of organizations.

Analysis of (3) produces interesting qualitative results. It is not difficult to show that a stable two-population equilibrium exists for the system in (3) only if

$$
\frac{1}{\alpha_{21}} < \frac{k_2}{k_1} < \alpha_{12}. \tag{4}
$$

Therefore, very similar populations (i.e., populations with competition coefficients near unity) can coexist only under a very precise k_2/k_1 ratio. As a result, when $\alpha_{12} = \alpha_{21} = 1$, no two-population equilibrium can be stable; any exogenous shock will result in the elimination of one of the populations. This result supports the generality of the widely cited "prin-

ciple of competitive exclusion" (Gause 1934).[7] According to this principle, no two populations can continuously occupy the same niche. Populations are said to occupy the same niche to the extent that they depend on identical environmental resources. If they are identical, then the addition of an element to X_2 has the same consequences for growth in X_1 as does the addition of an element to X_1; in other words, the competition coefficients are unity. The broad conclusion is that the greater the similarity of two resource-limited competitors, the less feasible is it that a single environment can support both of them in equilibrium.

If two populations of organizations sustained by identical environmental resources differ in some organizational characteristic, that population with the characteristic less fit to environmental contingencies will tend to be eliminated. The stable equilibrium will then contain only one population which can be said to be isomorphic to the environment.

In order to see the implications of the model for organizational diversity, we extend the Lotka-Volterra system to include M competitors:

$$\frac{dX_i}{dt} = r_i X_i \,(k_i - X_i - \Sigma\alpha_{ij}X_j)/k_i \quad (i = 1, \ldots, M). \tag{5}$$

The general system (5) has a community equilibrium:

$$k_i = X_i + \Sigma\alpha_{ij}X_j \qquad (i = 1, \ldots, M). \tag{6}$$

These equations can be expressed in matrix form:

$$k = A\,x, \tag{7}$$

where x and k are $(M \times 1)$ column vectors and A is the community matrix:

$$A = \begin{pmatrix} 1 & \alpha_{12} & \cdot & \cdot & \alpha_{1m} \\ \alpha_{21} & 1 & & & \cdot \\ \cdot & & & & \cdot \\ \cdot & & & & \cdot \\ \cdot & & & & \cdot \\ \alpha_{m1} & \cdot & \cdot & \cdot & 1 \end{pmatrix}$$

whose elements are the competition coefficients.

The so-called theory of community structure entails the analysis of the equilibrium behavior of the system of equation (7) from the perspective of postulated competition processes.[8] The results, though stated in

[7] This so-called principle has mostly suggestive value (see MacArthur [1972, pp. 43–46] for a penetrating critique of attempts to derive quantitative implications from Gause's principle; most of these criticisms do not apply to the qualitative inferences we consider).

[8] We restrict attention to the case in which all entries of A are nonnegative. Nega-

terms of species diversity, are quite general. In particular, one can show that when growth in population is constrained only by resource availability, the number of distinct resources sets an upper bound on diversity in the system.[9] Even more generally, the upper bound on diversity is equal to the number of distinct resources plus the number of additional constraints on growth (Levin 1970).

It is difficult to apply either result directly in order to calculate the upper bound on diversity even in the nonhuman context. The chief difficulty is that of identifying distinct constraints. A good deal of empirical work is required if one is to judge how different two constraints must be in order to have distinct consequences for community equilibria. The theorems do, however, imply useful qualitative results. If one can identify environmental changes which add constraints to a system or eliminate them, one can conclude that the upper bound of diversity is increased or decreased.

This broad qualitative result has a number of potential applications to the research problems of interest. For example, the expansion of markets and of state control mechanisms through social systems tends to have the consequence of eliminating or reducing the number of constraints which are idiosyncratic to local environments. Viewed from the perspective of the larger system, the process of expansion of the economic and political center should, then, tend to replace some local constraints with more uniform ones. As long as the local environments were heterogeneous at the outset, expansion of the center ought to reduce the number of constraints on organization in the whole system.

The theory just discussed implies on the one hand that the change in constraint structure ought to lower organizational diversity through the elimination of some population.[10] One can imagine, on the other hand, that in some local environments, the combination of unaltered local constraints and new larger system constraints might increase the total number of constraints in the local system. In that case, organizational diversity in those local environments should increase. Such an increase would result in the creation or adoption of new organizational forms.

The increasingly important role of the state in regulating economic and social action provides numerous opportunities for analyzing the impact of changes in constraint structures on the diversity of organizational

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tive entries are appropriate for predator/prey (or more generally, host/parasite) relations. The typical result for this case is cyclical population growth

[9] A more precise statement of the theorem is that no stable equilibrium exists for a system of M competitors and $N < M$ resources (MacArthur and Levins 1964).

[10] For a more comprehensive statement of this argument with reference to ethnic organization, see Hannan (1975).

forms. Consider the impact of licensing laws, minimum wage, health, and safety legislation, affirmative action, and other regulations on organizational action. When such regulations are applied to the full range of organizations in broad areas of activity they undoubtedly alter the size distributions of organizations. Most often they select out the smallest organizations. But it is not difficult to imagine situations in which medium-sized organizations (more precisely, those with some minimum level of complexity) would be more adversely affected. Besides altering size distributions, such regulations undoubtedly affect the diversity of organizational arrangements in other ways. Here one could analyze the impact of state action on the diversity of accounting systems within industries, curricula within universities, departmental structures within hospitals, etc. In each case it would be essential to determine whether the newly imposed constraint replaced lower level constraints, in which case diversity should decline, or whether the constraint cumulated with the existing constraints, in which case organizational diversity would be likely to increase.

To indicate the richness of the simple competition theory we have proposed we will briefly discuss another sort of empirical test. We noted above that research on regulation might concern itself with impacts on distributions of organizations by size. The classical model of organizational size distributions (Simon and Bonini 1958) proposes the following simple process. A number of organizations begin with the same small size. Some fraction are able to make or borrow some useful technical or organizational innovation that permits them to grow to some larger size. During some specified time period the process repeats itself with the same fraction making the innovation required to attain a larger size. Such a growth process eventually yields the lognormal distribution that characterizes so many size distributions.

Competition theory suggests a refinement of this classical model. If, as we argued earlier, large changes in organizational size are accompanied by structural changes (changes in form), organizations of very different size in the same area of activity will tend to exhibit different forms. As a consequence of these structural differences, they will tend to depend on different sets of environmental resource (and constraints). That is, within any area of activity, patterns of resource use will tend to be specialized to segments of the size distribution. This being the case, organizations will compete most intensely with similar size organizations. Also, competition between pairs of organizations within an activity will be a decreasing function of the distance separating them on the size gradient. For example, small local banks compete most with other small banks, to a lesser extent with medium-scale regional banks, and hardly at all with international banks. Under these conditions, significant alterations in the

size distribution indicate selection for and against certain organizational forms closely associated with regard to size.

Now let us return to the classical model. When large-sized organizations emerge they pose a competitive threat to medium-sized but hardly any threat to small organizations. In fact, the rise of large organizations may increase the survival chances of small ones in a manner not anticipated in the classical model. When the large organizations enter, those in the middle of the size distribution are trapped. Whatever strategy they adopt to fight off the challenge of the larger form makes them more vulnerable in competition with small organizations and vice versa. That is, at least in a stable environment the two ends of the size distribution ought to outcompete the middle (see below). So in a longitudinal analysis of organizational size distributions we would expect to see the number of medium-sized organizations decline upon the entry of larger organizations. Also, we would expect the fortunes of small organizations to improve as their competitors are removed from the environment. This reasoning holds generally for competition along a single gradient: those in the middle will be eliminated in stable environments (MacArthur 1972, pp. 43–46).

VI. NICHE THEORY

The principle of isomorphism implies that social organizations in equilibrium will exhibit structural features that are specialized to salient features of the resource environment. As long as the environment is stable and certain, we see no difficulty with this proposition. But does it hold when the environment shifts either predictably or unpredictably among several alternative configurations? Though the issues raised by attempting to answer this question are complex, doing so is crucial to developing adequate models of organizational-environment relations.

Intuition suggests that isomorphism holds as a good approximation only in stable environments. Faced with unstable environments, organizations ought to develop a generalist structure that is not optimally adapted to any single environmental configuration but is optimal over an entire set of configurations. In other words, we ought to find specialized organizations in stable and certain environments and generalist organizations is unstable and uncertain environments. Whether or not this simple proposition holds for social organizations, only empirical research will tell. However, a variety of population ecology models suggests that it is too simplistic. We cannot hope in one paper to develop fully the arguments involved. Instead we indicate the main lines of development with reference to one rather evocative perspective developed by Levins (1962, 1968): the theory of niche width.

The concept of "niche," initially borrowed by biologists from early

social science, plays a central role in ecological theory. This is not the place for an extended discussion of the multiple uses of the concept (see Whittaker and Levin 1976). The model which follows uses Hutchinson's (1957) formulation. From this point of view the (realized) niche of a population is defined as that area in constraint space (the space whose dimensions are levels of resources, etc.) in which the population outcompetes all other local populations. The niche, then, consists of all those combinations of resource levels at which the population can survive and reproduce itself.

Each population occupies a distinct niche. For present purposes it suffices to consider cases where pairs of populations differ with respect to a single environmental dimension, *E*, and are alike with respect to all others. Then relative competitive positions can be simply summarized as in figure 1. As we have drawn this figure, one population, *A*, occupies a

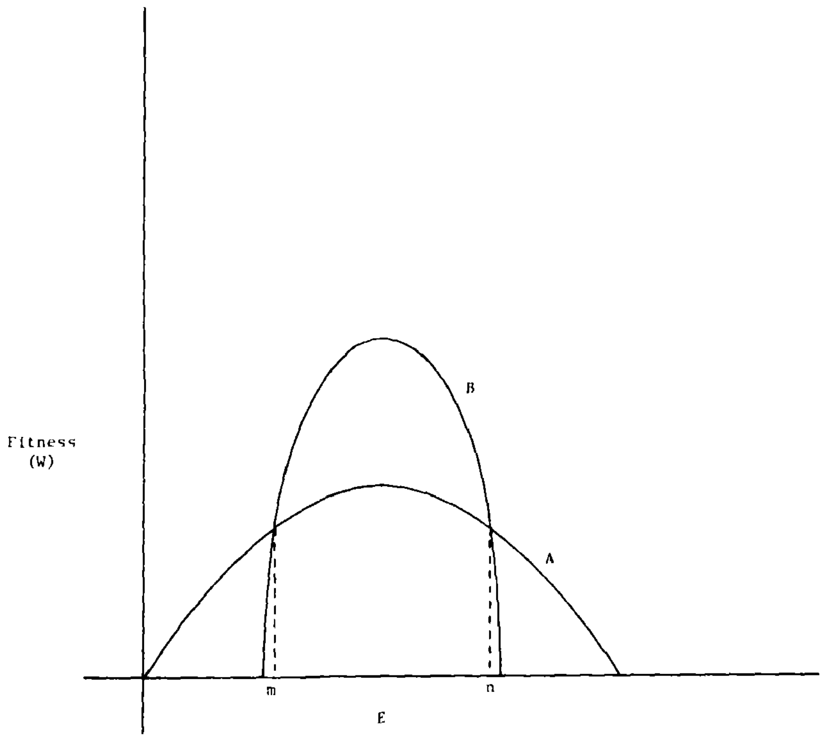


FIG. 1.—Fitness functions (niches) for specialists and generalists

very broad niche, whereas the other, *B*, has concentrated its fitness, denoted *W*, on a very narrow band of environmental variation. This distinction, which is usually referred to as generalism versus specialism, is crucial to biological ecology and to a population ecology of organizations.

In essence, the distinction between specialism and generalism refers to whether a population of organizations flourishes because it maximizes its exploitation of the environment and accepts the risk of having that environment change or because it accepts a lower level of exploitation in return for greater security. Whether or not the equilibrium distribution of organizational forms is dominated by the specialist depends, as we will see, on the shape of the fitness sets and on properties of the environment.

Part of the efficiency resulting from specialism is derived from the lower requirements for excess capacity. Given some uncertainty, most organizations maintain some excess capacity to insure the reliability of performance. In a rapidly changing environment, the definition of excess capacity is likely to change frequently. What is used today may become excess tomorrow, and what is excess today may be crucial tomorrow. Organizations operating in environments where the transition from state to state is less frequent will (in equilibrium) have to maintain excess capacity in a given allocational pattern for longer periods of time. Whereas those charged with assessing performance will be tempted to view such allocations as wasteful, they may be essential for survival. Thompson (1967) has argued that organizations allocate resources to units charged with the function of insulating core technology from environmentally induced disruption. So, for example, manufacturing firms may retain or employ legal staffs even when they are not currently facing litigation.

The importance of excess capacity is not completely bound up with the issue of how much excess capacity will be maintained. It also involves the manner in which it is used. Organizations may insure reliable performance by creating specialized units, as Thompson (1967) suggests, or they may allocate excess capacity to organizational roles, by employing personnel with skills and abilities which exceed the routine requirements of their jobs. This is one of the important reasons for using professionals in organizations. Professionals use more resources not only because they tend to be paid more, but also because organizations must allow them more discretion (including the freedom to respond to outside reference groups). Organizations, in turn, become more flexible by employing professionals. They increase their capacity to deal with a variable environment and the contingencies it produces. For example, hospitals and their patients often employ obstetricians and pediatricians in their delivery rooms even though the normal delivery of babies can be performed equally well, and perhaps even better, by midwives. The skills of the medical doctor represent excess capacity to insure reliable performance should delivery not be normal. Usually, the pediatrician examines the infant immediately after birth to see if there is any abnormality requiring immediate action. If the mother is suffering dangerous consequences from giving birth, and the child is also in need of attention, the presence of the

pediatrician insures that the obstetrician will not have to choose between them in allocating his attention.

Excess capacity may also be allocated to the development and maintenance of procedural systems. When the certainty of a given environmental state is high, organizational operations should be routine, and coordination can be accomplished by formalized rules and the investment of resources in training incumbents to follow those formalized procedures. If in fact the environment were unchanging ($p = 1$), all participants were procedurally skilled, and the procedures were perfectly tuned, there would be no need for any control structure at all, except to monitor behavior. However, when certainty is low, organizational operations are less routine. Under these circumstances, a greater allocation of resources to develop and maintain procedural systems is counterproductive and optimal organizational forms will allocate resources to less formalized systems capable of more innovative responses (e g , committees and teams). In this case, excess capacity is represented by the increased time it takes such structures to make decisions and by increased coordination costs.

The point here is that populations of organizational forms will be selected for or against depending upon the amount of excess capacity they maintain and how they allocate it. It may or may not be rational for any particular organization to adopt one pattern or another. What would seem like waste to anyone assessing performance at one time may be the difference between survival and failure later. Similarly, organizations may survive because high levels of professionalization produce coordination by mutual adjustment despite a somewhat chaotic appearance. Others, in which everyone seems to know precisely what he is doing at all times, may fail. Under a given set of environmental circumstances the fundamental ecological question is: which forms thrive and which forms disappear.

Generalism may be observed in a population of organizations, then, either in its reliance upon a wide variety of resources simultaneously or in its maintenance of excess capacity at any given time. This excess capacity allows such organizations to change in order to take advantage of resources which become more readily available. Corporations which maintain an unusually large proportion of their total assets in fluid form ("slack," in terms of theory of the firm; Penrose 1959; Cyert and March 1963) are generalizing. In either case, generalism is costly. Under stable environmental circumstances, generalists will be outcompeted by specialists. And at any given point in time, a static analysis will reveal excess capacity. An implication—shifting our focus to individual generalists—is that outside agents will often mistake excess capacity for waste.

We can investigate the evolution of niche width if we make the assumption that areas under the fitness curve are equal, and that specialists differ from generalists in how they distribute the fixed quantity of fitness over environmental outcomes. Specialists outcompete generalists over the range of outcomes to which they have specialized (because of the fixed level of fitness assumption). As long as the environmental variation remains within that interval (the interval $[m,n]$ in fig. 1), generalists have no adaptive advantage and will be selected against. Alternatively, if the environment is only occasionally within the interval, specialists will fare less well than generalists. These brief comments make clear the importance of environmental variation for the evolution of niche width.

To simplify further, consider an environment which can take on only two states and in every period falls in state one with probability p and in state two with probability $q = (1 - p)$. Assume further that variations in environmental states are Bernoulli trials (independent from period to period). For this situation Levins (1962, 1968) has shown that optimal niche width depends on p and the "distance" between the two states of the environment.

To see this, we change focus slightly. Since each organization faces two environments, its fitness depends on fitness in the pair. We can summarize the adaptive potential of each organization by plotting these pairs of values (fitness in state 1 and in state 2) in a new space whose axes are fitness in each of the states, as in figure 2. In this representation, each point denotes the fitness of a distinct organizational form. The cloud of points is termed the "fitness set." We presume that all of the naturally possible adaptations are represented in the fitness set.

Our interest is in determining which points in the fitness set will be favored by natural selection. Notice first that all points interior to the set are inferior in terms of fitness to at least some point on the boundary of the set. In this sense the boundary, drawn as a continuous line, represents the optimal possibilities Since natural selection maximizes fitness, it must choose points on the boundary. This narrows our search to seeking which form(s) on the boundary will be favored.

As figure 2*b* is drawn, no organizational form does particularly well in both states of the environment—no form has high levels of fitness in both. This will be the case when the two states are "far apart" in the sense that they impose very different adaptive contingencies on organizations. In such cases (see Levins 1968), the fitness set will be concave. When the "distance" between states is small, there is no reason why certain organizational forms cannot do well in both environments. In such cases, the finess set will be convex, as in figure 2*a*.

The fitness functions in figures 2*a* and 2*b* describe different adaptive situations. The next step is to model the optimization process. To do so,

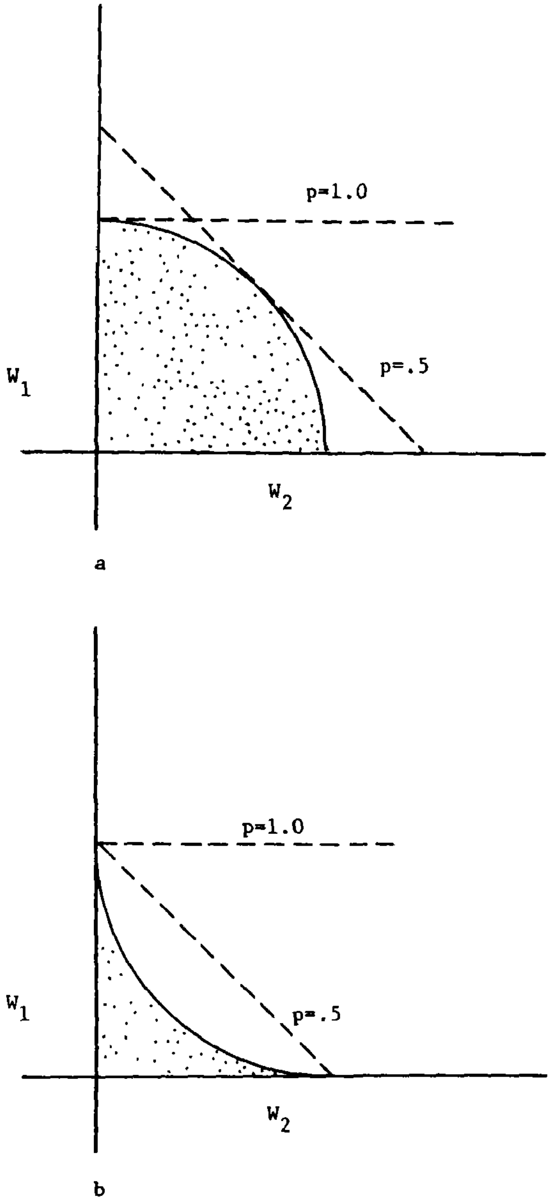


FIG 2.—Optimal adaptation in fine-grained environment; *a*, convex fitness set, *b*, concave fitness set

we introduce a further distinction. Ecologists have found it useful to distinguish both spatial and temporal environmental variation according to grain. Environmental variation is said to be fine-grained when a typical element (organization) encounters many units or replications. From

a temporal perspective, variation is fine-grained when typical durations in states are short relative to the lifetime of organizations. Otherwise, the environment is said to be coarse-grained. Demand for products or services is often characterized by fine-grained variation whereas changes in legal structures are more typically coarse-grained.

The essential difference between the two types of environmental variation is the cost of suboptimal strategies. The problem of ecological adaptation can be considered a game of chance in which the population chooses a strategy (specialism or generalism) and then the environment chooses an outcome (by, say, flipping a coin). If the environment "comes up" in a state favorable to the organizational form, it prospers; otherwise, it declines. However, if the variation is fine-grained (durations are short), each population of organizations experiences a great many trials and environment is experienced as an average. When variation is coarse-grained, however, the period of decline stemming from a wrong choice may exceed the organizational capacity to sustain itself under unfavorable conditions

To capture these differences, Levins introduced an adaptive function to represent how natural selection would weight fitness in each state under the different conditions. In discussing fine-grained variation, we suggested that the environment is experienced as an average.[11] The appropriate adaptive function, then, simply weights fitness in the two states (W_1 and W_2) according to frequency of occurrence: $A(W_1,W_2) = pW_1 + qW_2$ In order to consider optimal adaptation we merely superimpose the adaptive function on the fitness set and find points of tangency of adaptive function and fitness functions. Points of tangency are optimal adaptations. The solutions for various cases are presented in figure 2. If the environment is completely stable (i.e., $p = 1$), then specialism is optimal. If the environment is maximally uncertain (i.e., $p = .5$), generalism is optimal in the convex case (when the demands of the different environments are not too dissimilar) but not in the concave case. In fact, as the model is developed, specialism always wins out in the concave case.

Consider first the cases in which the environment is stable (i.e., $p = 1$). Not surprisingly, specialism is optimal. The results for unstable environments diverge. When the fitness set is convex (i.e., the demands of the different environmental states are similar and/or complementary), generalism is optimal. But when the environmental demands differ (and the fitness set is concave), specialism is optimal. This is not as strange

[11] That selection depends on average outcomes is only one hypothesis Templeton and Rothman (1974) argue that selection depends not on average outcomes but on some minimum level of fitness. Whether average outcomes or some other criterion guides selection in populations of organizations is open to question. We follow Levins in order to keep the exposition simple.

a result as it first appears. When the environment changes rapidly among quite different states, the cost of generalism is high. Since the demands in the different states are dissimilar, considerable structural management is required of generalists. But since the environment changes rapidly, these organizations will spend most of their time and energies adjusting structure. It is apparently better under such conditions to adopt a specialized structure and "ride out" the adverse environments.

The case of coarse-grained environments is somewhat more complex. Our intuitive understanding is that since the duration of an environmental state is long, maladaptation ought to be given greater weight. That is, the costs of maladaptation greatly outweigh any advantage incurred by the correct choice. One adaptive function which gives this result is the log-linear model used by Levins: $A(W_1,W_2) = W_1^pW_2^q$. The method of finding optimal adaptations is the same. The results are found in figure 3. Only one case differs from what we found for fine-grained environments: the combination of uncertain and coarse-grained variation with concave fitness sets. We saw above that when such variation is fine-grained, it is better to specialize. When the duration of environmental states is long, however, the costs of this strategy are great Long periods of nonadaptation will threaten the survival of the organization. In addition, the fact that the environment changes less often means that generalists need not spend most of their time and energies altering structure. Thus generalism is the optimal strategy in this case as we see in figure *3b*.

The combination of coarse-grained environmental variation and concave fitness sets raises a further possibility. The optimal adaptation in the face of environmental uncertainty possesses fairly low levels of fitness in either state. It seems clear that there must be a better solution. Levins discusses this case in depth and concludes that for the biological case with genetic transmission of structure "polymorphism" or genetically maintained population heterogeneity will be selected for. The suggestion is that populations combine types (differing, say, in color, blood type, etc.) some of which are specialized to state 1 and some to state 2. With such a combination at least a portion of the population will always flourish and maintain the genetic diversity which allows it to continue to flourish when the environment changes state. The set of all such heterogeneous populations (composed of proportions of specialists to each of the two environments) can be represented in the fitness diagrams as a straight line joining the most extreme points with all combinations falling within this line.

Coarse-grained and uncertain variation favors a distinct form of generalism: polymorphism. We do not have to search very far to find an analogous outcome. Organizations may federate in such a way that supraorganizations consisting of heterogeneous collections of specialist organi-

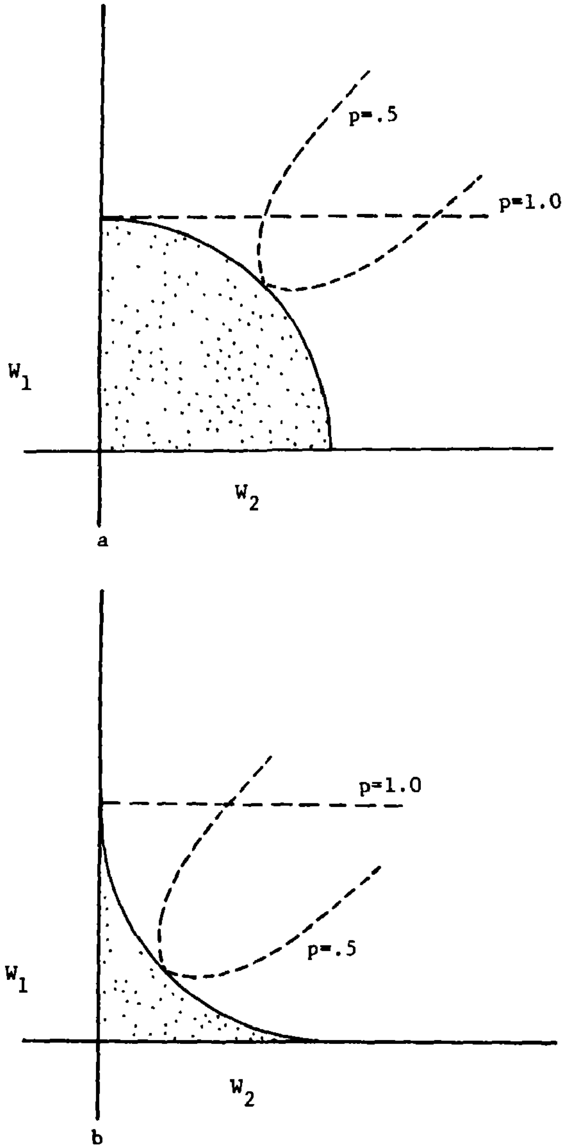


FIG. 3 —Optimal adaptation in coarse-grained environments; *a*, convex fitness set; *b*, concave fitness set.

zations pool resources. When the environment is uncertain and coarse-grained and subunits difficult to set up and tear down, the costs of maintaining the unwieldy structure imposed by federation may be more than offset by the fact that at least a portion of the amalgamated organization will do well no mattet what the state of the environment. In terms of the model suggested above there are no other situations in which such

federated organizations have a competitive advantage. And even in this case, the only time during which they have such an advantage is when coarse-grained variation is uncertain.

Such an amalgamated "holding company" pattern may be observed in modern universities. Enrollment and research support wax and wane over time as do the yield on invested endowment securities and the beneficence of legislatures. Some of these resources follow predictable cycles. Others do not. But it is extremely expensive to build up and dismantle academic units. It is costly not only in money but also in the energies consumed by political conflict. Consequently, universities are constantly "taxing" subunits with plentiful environments to subsidize less fortunate subunits. It is common, for instance, for universities to allocate faculty positions according to some fixed master plan, undersupporting the rapidly growing departments and maintaining excess faculty in others. This partial explanation of the unwieldly structures that encompass liberal arts departments, professional schools, research laboratories, etc., is at least as persuasive as explanations that emphasize intellectual interdependence among units.

Much more can be said concerning applications of niche theory to organization-environment relations. We have focused on a simple version highlighting the interplay between competition and environmental variation in the determination of optimal adaptive structure in order to show that the principle of isomorphism needs considerable expansion to deal with multiple environmental outcomes and their associated uncertainty. The literature in ecology to which we have made reference is growing exponentially at the moment and new results and models are appearing monthly. The products of these developments provide students of organizations with a rich potential for the study of organization-environment relations.

Consider an example. In his analysis of bureaucratic and craft administration or production, Stinchcombe (1959) argued that construction firms do not rely upon bureaucratically organized administrative staffs because of seasonal fluctuations in demand. Administrative staffs constitute an overhead cost which remains roughly constant over the year. The advantage of the otherwise costly (in terms of salaries) craft administration is that coordination of work is accomplished through a reliance upon prior socialization of craftsmen and upon organization. Since employment levels can more easily be increased or decreased with demand under a craft system, administrative costs are more easily altered to meet demand.

The fundamental source of this pattern is the seasonal variation in construction. In ecological terms, the demand environment is coarse-grained. In addition, the two states defined by season are quite different, resulting in a concave fitness curve. Craft-administered housing construction firms are probably quite inefficient when demand is at its peak and

when the kind of housing under construction is standardized. In such situations, we would expect this form of organization to face stiff competition from other firms. For instance, in regions where housing construction is less seasonal, modular housing, mobile homes, and prefabricated housing are more likely to flourish and we would expect the construction business to be more highly bureaucratized.

Another variation in demand is to be found in the business cycle. While seasonal fluctuations are stable (uncertainty is low), interest rates, labor relations, and materials costs are more difficult to predict. Variations of this sort should favor a generalist mode of adaptation. That is, when environments are coarse-grained, characterized by concave fitness curves, and uncertain, populations of organizations will be more likely to survive if they hedge their bets by seeking a wider variety of resource bases. For this reason, we think, craft-administered construction organizations are frequently general contractors who not only build houses but engage in other kinds of construction as well (shopping plazas, office buildings, etc.). In comparison, modular housing is cheaper and the units are installed on rented space. Consequently, interest rates are less important. Since organizations producing this kind of housing do not employ craftsmen but use the cheapest and least skilled labor they can obtain, labor relations are less problematical. It may also be that their reliance on different materials (e.g., sheet aluminum) contributes to a lower level of uncertainty. In consequence, we would expect this form of organization to be more highly specialized in its adaptation (of course there are technical factors which also contribute to this as well).

Craft-administered construction firms are set up in such a way that they can adapt rapidly to changes in demand, and they can adapt to different construction problems by varying the mix of skills represented in their work force. Bureaucratically administered construction firms are more specialized and as a result they are efficient only when demand is high, and very inefficient when it is low. We also believe that they tend to be more specialized with regard to type of construction. Craft-administered organizations sacrifice efficient exploitation of their niche for flexibility. Bureaucratic organizations choose the opposite strategy. This formulation is an extension of Stinchcombe's and serves to show that his argument is essentially ecological.

VII. DISCUSSION

Our aim in this paper has been to move toward an application of modern population ecology theory to the study of organization-environment relations. For us, the central question is, why are there so many kinds of organizations? Phrasing the question in this way opens the possibility

of applying a rich variety of formal models to the analysis of the effects of environmental variations on organizational structure.

We begin with Hawley's classic formulation of human ecology. However, we recognize that ecological theory has progressed enormously since sociologists last systematically applied ideas from bioecology to social organization. Nonetheless, Hawley's theoretical perspective remains a very useful point of departure. In particular we concentrate on the principle of isomorphism. This principle asserts that there is a one-to-one correspondence between structural elements of social organization and those units that mediate flows of essential resources into the system. It explains the variations in organizational forms in equilibrium. But any observed isomorphism can arise from purposeful adaptation of organizations to the common constraints they face or because nonisomorphic organizations are selected against Surely both processes are at work in most social systems. We believe that the organizations literature has emphasized the former to the exclusion of the latter.

We suspect that careful empirical research will reveal that for wide classes of organizations there are very strong inertial pressures on structure arising both from internal arrangements (e g., internal politics) and the environment (e.g., public legitimation of organizational activity). To claim otherwise is to ignore the most obvious feature of organizational life. Failing churches do not become retail stores; nor do firms transform themselves into churches. Even within broad areas of organizational action, such as higher education and labor union activity, there appear to be substantial obstacles to fundamental structural change. Research is needed on this issue. But until we see evidence to the contrary, we will continue to doubt that the major features of the world of organizations arise through learning or adaptation. Given these doubts, it is important to explore an evolutionary explanation of the principle of isomorphism. That is, we wish to embed the principle of isomorphism within an explicit selection framework.

In order to add selection processes we propose a competition theory using Lotka-Volterra models. This theory relies on growth models that appear suitable for representing both organizational development and the growth of populations of organizations. Recent work by bioecologists on Lotka-Volterra systems yields propositions that have immediate relevance for the study of organization-environment relations. These results concern the effects of changes in the number and mixture of constraints upon systems with regard to the upper bound of the diversity of forms of organization. We propose that such propositions can be tested by examining the impact of varieties of state regulation both on size distributions and on the diversity of organizational forms within broadly defined areas of activity (e.g., medical care, higher education, and newspaper publishing).

A more important extension of Hawley's work introduces dynamic considerations. The fundamental issue here concerns the meaning of isomorphism in situations in which the environment to which units are adapted is changing and uncertain. Should "rational" organizations attempt to develop specialized isomorphic structural relations with one of the possible environmental states? Or should they adopt a more plastic strategy and institute more generalized structural features? The isomorphism principle does not speak to these issues.

We suggest that the concrete implication of generalism for organizations is the accumulation and retention of varieties of excess capacity. To retain the flexibility of structure required for adaptation to different environmental outcomes requires that some capacities be held in reserve and not committed to action. Generalists will always be outperformed by specialists who, with the same levels of resources, happen to have hit upon their optimal environment. Consequently, in any cross-section the generalists will appear inefficient because excess capacity will often be judged waste. Nonetheless, organizational slack is a pervasive feature of many types of organizations. The question then arises: what types of environments favor generalists? Answering this question comprehensively takes one a long way toward understanding the dynamic of organization-environment relations.

We begin addressing this question in the suggestive framework of Levins's (1962, 1968) fitness-set theory. This is one of a class of recent theories that relates the nature of environmental uncertainty to optimal levels of structural specialism. Levins argues that along with uncertainty one must consider the grain of the environment or the lumpiness of environmental outcomes. The theory indicates that specialism is always favored in stable or certain environments. This is no surprise. But contrary to the view widely held in the organizations literature, the theory also indicates that generalism is not always optimal in uncertain environments. When the environment shifts uncertainly among states that place very different demands on the organization, and the duration of environmental states is short relative to the life of the organization (variation is fine-grained), populations of organizations that specialize will be favored over those that generalize. This is because organizations that attempt to adapt to each environmental outcome will spend most of their time adjusting structure and very little time in organizational action directed at other ends.

Stated in these terms, the proposition appears obvious. However, when one reads the literature on organization-environment relations, one finds that it was not so obvious. Most important, the proposition follows from a simple explicit model that has the capacity to unify a wide variety of propositions relating environmental variations to organizational structure.

We have identified some of the leading conceptual and methodological obstacles to applying population ecology models to the study of organization-environment relations. We pointed to differences between human and nonhuman social organization in terms of mechanisms of structural invariance and structural change, associated problems of delimiting populations of organizations, and difficulties in defining fitness for populations of expandable units. In each case we have merely sketched the issues and proposed short-run simplifications which would facilitate the application of existing models. Clearly, each issue deserves careful scrutiny.

At the moment we are frustrated at least as much by the lack of empirical information on rates of selection in populations of organizations as by the unresolved issues just mentioned. Census data are presented in a manner that renders the calculation of failure rates impossible; and little longitudinal research on populations of organizations has been reported. We do, however, have some information on rates of selection. We know, for example, that failure rates for small businesses are high. By recent estimates upwards of 8% of small business firms in the United States fail each year (Hollander 1967; Bolton 1971; see also Churchill 1955).

In part this high failure rate reflects what Stinchcombe (1965) called the liability of newness. Many new organizations attempt to enter niches that have already been filled by organizations that have amassed social, economic, and political resources that make them difficult to dislodge. It is important to determine whether there is any selective disadvantage of smallness not of newness.

We doubt that many readers will dispute the contention that failure rates are high for new and/or small organizations. However, much of the sociological literature and virtually all of the critical literature on large organizations tacitly accepts the view that such organizations are not subject to strong selection pressures. While we do not yet have the empirical data to judge this hypothesis, we can make several comments. First, we do not dispute that the largest organizations individually and collectively exercise strong dominance over most of the organizations that constitute their environments. But it does not follow from the observation that such organizations are strong in any one period that they will be strong in every period. Thus, it is interesting to know how firmly embedded are the largest and most powerful organizations. Consider the so-called Fortune 500, the largest publicly owned industrial firms in the United States. We contrasted the lists for 1955 and 1975 (adjusting for pure name changes). Of those on the list in 1955, only 268 (53.6%) were still listed in 1975. One hundred twenty-two had disappeared through merger, 109 had slipped off the "500," and one (a firm specializing in Cuban sugar!) had been liquidated. The number whose relative sales

growth caused them to be dropped from the list is quite impressive in that the large number of mergers had opened many slots on the list. So we see that, whereas actual liquidation was rare for the largest industrial firms in the United States over a 20-year period, there was a good deal of volatility with regard to position in this pseudodominance structure because of both mergers and slipping sales.[12]

Second, the choice of time perspective is important. Even the largest and most powerful organizations fail to survive over long periods. For example, of the thousands of firms in business in the United States during the Revolution, only 13 survive as autonomous firms and seven as recognizable divisions of firms (*Nation's Business* 1976). Presumably one needs a longer time perspective to study the population ecology of the largest and most dominant organizations.

Third, studying small organizations is not such a bad idea. The sociological literature has concentrated on the largest organizations for obvious design reasons. But, if inertial pressures on certain aspects of structure are strong enough, intense selection among small organizations may greatly constrain the variety observable among large organizations. At least some elements of structure change with size (as we argued in Section III) and the pressure toward inertia should not be overemphasized. Nonetheless we see much value in studies of the organizational life cycle that would inform us as to which aspects of structure get locked in during which phases of the cycle. For example, we conjecture that a critical period is that during which the organization grows beyond the control of a single owner/manager. At this time the manner in which authority is delegated, if at all, seems likely to have a lasting impact on organizational structure. This is the period during which an organization becomes less an extension of one or a few dominant individuals and more an organization per se with a life of its own. If the selection pressures at this point are as intense as anecdotal evidence suggests they are, selection models will prove very useful in accounting for the varieties of forms among the whole range of organizations.

The optimism of the previous paragraph should be tempered by the realization that when one examines the largest and most dominant organizations, one is usually considering only a small number of organizations. The smaller the number, the less useful are models that depend on the type of random mechanisms that underlie population ecology models.

Fourth, we must consider what one anonymous reader, caught up in the spirit of our paper, called the anti-eugenic actions of the state in

[12] From at least some perspectives, mergers can be viewed as changes in form. This will almost certainly be the case when the organizations merged have very different structures. These data also indicate a strong selective advantage for a conglomerate form of industrial organization.

saving firms such as Lockheed from failure. This is a dramatic instance of the way in which large dominant organizations can create linkages with other large and powerful ones so as to reduce selection pressures. If such moves are effective, they alter the pattern of selection. In our view the selection pressure is bumped up to a higher level. So instead of individual organizations failing, entire networks fail. The general consequence of a large number of linkages of this sort is an increase in the instability of the entire system (Simon 1962, 1973; May 1973), and therefore we should see boom and bust cycles of organizational outcomes. Selection models retain relevance, then, even when the systems of organizations are tightly coupled (see Hannan 1976).

Finally, some readers of earlier drafts have (some approvingly, some disapprovingly) treated our arguments as metaphoric. This is not what we intend. In a fundamental sense all theoretical activity involves metaphoric activity (although admittedly the term "analogue" comes closer than does "metaphor"). The use of metaphors or analogues enters into the formulation of "if . . . then" statements For example, certain molecular genetic models draw an analogy between DNA surfaces and crystal structures. The latter have simple well-behaved geometric structures amenable to strong topological (mathematical) analysis No one argues that DNA proteins are crystals; but to the extent that their surfaces have certain crystal-like properties, the mathematical model used to analyze crystals will shed light on the genetic structure. This is, as we understand it, the general strategy of model building.

We have, for example, used results that rely on the application of certain logistic differential equations, the Lotka-Volterra equations. No known population (of animals, or of organizations) grows in exactly the manner specified by this mathematic model (and this fact has caused numerous naturalists to argue that the model is biologically meaningless). What the equations do is to model the growth path of populations that exist on finite resources in a closed system (where population growth in the absence of competition is logistic and the presence of competing populations lowers carrying capacities in that system) To the extent that the interactions of populations of *Paramecium aurcilia* and *P. caudatum* (Gause's experiment) meet the conditions of the model, the model explains certain key features of population dynamics and the relationship of environmental variations to structure. To the extent that the interactions of populations of rational-legal bureaucracies and populations of patrimonial bureaucracies also meet the conditions of the model, the model explains the same important phenomena. Neither the protozoa nor the bureaucracies behave exactly as the model stipulates. The model is an abstraction that will lead to insight whenever the stated conditions are approximated.

Throughout we make a strong continuity-of-nature hypothesis. We propose that, whenever the stated conditions hold, the models lead to valuable insights regardless of whether the populations under study are composed of protozoans or organizations. We do not argue "metaphorically." That is, we do *not* argue as follows: an empirical regularity is found to hold for certain protozoans; because we hypothesize that populations of organizations are like populations of protozoans in essential ways, we propose that the generalizations derived from the latter will hold for organizations as well. This is the kind of reasoning by which biological propositions have most often entered sociological arguments (e.g., the famous—or infamous—organismic analogy advanced by Spencer).

Instead of applying biological laws to human social organization, we advocate the application of population ecology theories. As we have indicated at a number of points, these theories are quite general and must be modified for any concrete application (sociological *or* biological). Our purpose has been twofold. First, we sketched some of the alterations in perspective required if population ecology theories are to be applied to the study of organizations. Second, we wished to stimulate a reopening of the lines of communication between sociology and ecology. It is ironic that Hawley's (1944, p. 399) diagnosis of some 30 years ago remains apt today: "Probably most of the difficulties which beset human ecology may be traced to the isolation of the subject from the mainstream of ecological thought."

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Some Effects of Proportions on Group Life: Skewed Sex Ratios and Responses to Token Women[1]

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Proportions, that is, *relative* numbers of socially and culturally different people in a group, are seen as critical in shaping interaction dynamics, and four group types are identified on the basis of varying proportional compositions. "Skewed" groups contain a large preponderance of one type (the numerical "dominants") over another (the rare "tokens"). A framework is developed for conceptualizing the processes that occur between dominants and tokens. Three perceptual phenomena are associated with tokens: visibility (tokens capture a disproportionate awareness share), polarization (differences between tokens and dominants are exaggerated), and assimilation (*tokens' attributes are distorted to fit preexisting generalizations* about their social type). Visibility generates performance pressures; polarization leads dominants to heighten their group boundaries; and assimilation leads to the tokens' role entrapment. Illustrations are drawn from a field study in a large industrial corporation. Concepts are extended to tokens of all kinds, and research issues are identified.

In his classic analysis of the significance of numbers in social life, Georg Simmel (1950) argued persuasively that numerical modifications effect qualitative transformations in group interaction. Simmel dealt almost exclusively with the impact of absolute numbers, however, with group size as a determinant of form and process. The matter of relative numbers, of proportion of interacting social types, was left unexamined. But this feature of collectivities has an impact on behavior. Its neglect has sometimes led to inappropriate or misleading conclusions.

This paper defines the issues that need to be explored. It addresses itself to proportion as a significant aspect of social life, particularly important for understanding interactions in groups composed of people of different cultural categories or statuses. It argues that groups with varying

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proportions of people of different social types differ qualitatively in dynamics and process. This difference is not merely a function of cultural diversity or "status incongruence" (Zaleznick, Christensen, and Roethlisberger 1958, pp. 56–68); it reflects the effects of contact across categories as a function of their proportional representation in the system.

Four group types can be identified on the basis of various proportional representations of kinds of people. *Uniform* groups have only one kind of person, one significant social type. The group may develop its own differentiations, of course, but groups considered uniform are homogeneous with respect to salient external master statuses such as sex, race, or ethnicity. Uniform groups have a "typological ratio" of 100:0. *Skewed* groups are those in which there is a large preponderance of one type over another, up to a ratio of perhaps 85:15. The numerically dominant types also control the group and its culture in enough ways to be labeled "dominants." The few of another type in a skewed group can appropriately be called "tokens," because they are often treated as representatives of their category, as symbols rather than individuals. If the absolute size of the skewed group is small, tokens can also be solitary individuals or "solos," the only one of their kind present. But even if there are two tokens in a skewed group, it is difficult for them to generate an alliance that can become powerful in the group. Next, *tilted* groups begin to move toward less extreme distributions and less exaggerated effects. In this situation, with a ratio of perhaps 65:35, dominants are just a majority and tokens a minority. Minority members are potentially allies, can form coalitions, and can affect the culture of the group. They begin to become individuals differentiated from each other as well as a type differentiated from the majority. Finally, at a typological ratio of about 60:40 down to 50:50, the group becomes *balanced*. Culture and interaction reflect this balance. Majority and minority turn into potential subgroups which may or may not generate actual type-based identifications. Outcomes for individuals in such a balanced peer group, regardless of type, will depend on other structural and personal factors, including formation of subgroups or differentiated roles and abilities. Figure 1 schematizes the four group types.

The characteristics of the second type, the skewed group, provide a relevant starting point for this examination of the effects of proportion, for although this group represents an extreme instance of the phenomenon, it is one encountered by large numbers of women in groups and organizations in which numerical distributions have traditionally favored men.

At the same time, this paper is oriented toward enlarging our understanding of male-female interaction and the situations facing women in

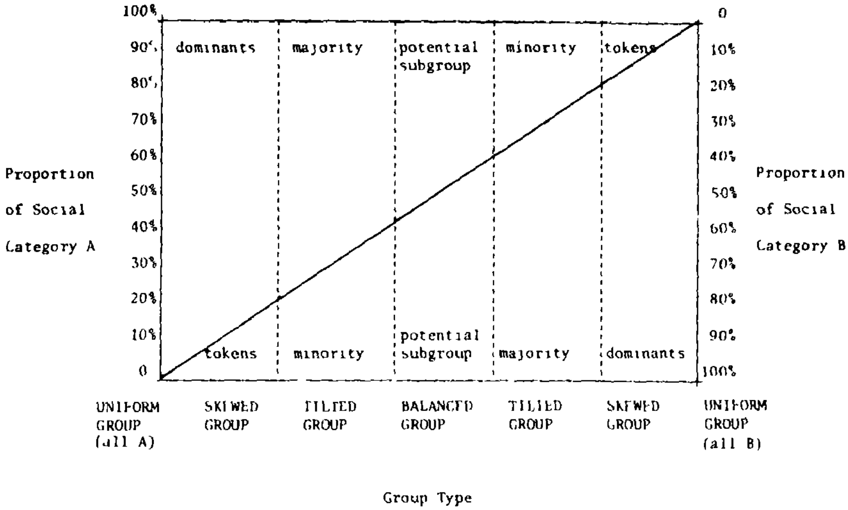


FIG. 1.—Group types as defined by proportional representation of two social categories in a membership.

organizations by introducing structural and contextual effects. Most analyses to date locate male-female interaction issues either in broad cultural traditions and the sexual division of labor in society or in the psychology of men and women whether based on biology or socialization (Kanter 1976*c*). In both macroscopic and microscopic analysis, sex and gender components are sometimes confounded by situational and structural effects. For example, successful women executives are almost always numerically rare in their organizations, whereas working women are disproportionately concentrated in low-opportunity occupations. Conclusions about "women's behavior" or "male attitudes" drawn from such situations may sometimes confuse the effect of situation with the effect of sex roles; indeed such variables as position in opportunity and power structures account for a large number of phenomena related to work behavior that have been labeled "sex differences" (Kanter 1975, 1976*a*, 1976*d*, and in press). Therefore this paper focuses on an intermediate-level analysis: how group structures shape interaction contexts and influence particular patterns of male-female interaction. One advantage of such an approach is that it is then possible to generalize beyond male-female relations to persons-of-one-kind and person-of-another-kind interaction in various contexts, also making possible the untangling of what exactly *is* unique about the male-female case.

The study of particular proportions of women in predominantly male groups is thus relevant to a concern with social organization and group process as well as with male-female interaction. The analysis presented

here deals with interaction in face-to-face groups with highly skewed sex ratios. More specifically, the focus is upon what happens to women who occupy token statuses and are alone or nearly alone in a peer group of men. This situation is commonly faced by women in management and the professions, and it is increasingly faced by women entering formerly all-male fields at every level of organizations. But proportional scarcity is not unique to women. Men can also find themselves alone among women, blacks among whites, very old people among the young, straight people among gays, the blind among the sighted. The dynamics of interaction (the process) is likely to be very similar in all such cases, even though the content of interaction may reflect the special culture and traditional roles of both token and members of the numerically dominant category.

Use of the term "token" for the minority member rather than "solo," "solitary," or "lone" highlights some special characteristics associated with that position. Tokens are not merely deviants or people who differ from other group members along any one dimension. They are people identified by ascribed characteristics (master statuses such as sex, race, religion, ethnic group, age, etc.) or other characteristics that carry with them a set of assumptions about culture, status, and behavior highly salient for majority category members. They bring these "auxiliary traits," in Hughes's (1944) term, into situations in which they differ from other people not in ability to do a task or in acceptance of work norms but only in terms of these secondary and informal assumptions. The importance of these auxiliary traits is heightened if members of the majority group have a history of interacting with the token's category in ways that are quite different from the demands of task accomplishment in the present situation—as is true of men with women. Furthermore, because tokens are by definition alone or virtually alone, they are in the position of representing their ascribed category to the group, whether they choose to do so or not. They can never be just another member while their category is so rare; they will always be a hyphenated member, as in "woman-engineer" or "male-nurse" or "black-physican."

People can thus be in the token position even if they have not been placed there deliberately for display by officials of an organization. It is sufficient to be in a place where others of that category are not usually found, to be the first of one's kind to enter a new group, or to represent a very different culture and set of interactional capacities to members of the numerically dominant category. The term "token" reflects one's status as a symbol of one's kind. However, lone people of one type among members of another are not necessarily tokens if their presence is taken for granted in the group or organization and incorporated into the dominant culture, so that their loneness is merely the accidental result of

random distributions rather than a reflection of the rarity of their type in that system.[2]

While the dynamics of tokenism are likely to operate in some form whenever proportional representation in a collectivity is highly skewed, even if the dominant group does not intend to put the token at a disadvantage, two conditions can heighten and dramatize the effects, making them more visible to the analyst· (1) the token's social category (master status) is physically obvious, as in the case of sex, and (2) the token's social type is not only rare but also new to the setting of the dominants. The latter situation may or may not be conceptually distinct from rarity, although it allows us to see the development of patterns of adjustment as well as the perception of and response to tokens. Subsequent tokens have less surprise value and may be thrust into token roles with less disruption to the system.

With only a few exceptions, the effects of differences in proportional representation within collectivities have received little previous attention. Hughes (1944, 1946, 1958) described the dynamics of white work groups entered by a few blacks, pointing out the dilemmas posed by status contradictions and illuminating the sources of group discomfort as they put pressures on the rare blacks. There are a few studies of other kinds of tokens such as Segal's (1962) observations of male nurses in a largely female colleague group. Reports of professional women in male-dominated fields (e.g., Epstein 1970; Hennig 1970; Lynch 1973; Cussler 1958) mention some special issues raised by numerical rarity. More recently, Laws (1975) has developed a framework for defining the induction of a woman into token status through interaction with a sponsor representing the numerically dominant group. Wolman and Frank (1975) reported observations of solo women in professional-training groups; Taylor and Fiske (1975) have developed experimental data on the perception of token blacks in a white male group. The material in all of these studies still needs a theoretical framework.

With the exceptions noted, research has generally failed to take into account the effects of relative numbers on interaction. Yet such effects could critically change the interpretation of familiar findings. The research of Strodtbeck and his colleagues (Strodtbeck and Mann 1956; Strodtbeck, James, and Hawkins 1957) on mock jury deliberations is often cited as

[2] As an anonymous reviewer pointed out, newness is more easily distinguished from rarity conceptually than it may be empirically, and further research should make this distinction. It should also specify the conditions under which "accidental loneness" (or small relative numbers) does not have the extreme effects noted here: when the difference is noted but not considered very important, as in the case of baseball teams that may have only one or two black members but lack token dynamics because of the large number of teams with many black members.

evidence that men tend to play initiating, task-oriented roles in small groups, whereas women tend to play reactive, socioemotional roles. Yet a reexamination of these investigations indicates that men far outnumbered women as research subjects. There were more than twice as many men as women (86 to 41) in the 12 small groups in which women were found to play stereotypical expressive roles.[3] The actual sex composition of each of the small groups is not reported, although it could have important implications for the results. Perhaps it was women's scarcity in skewed groups that pushed them into classical positions and men's numerical superiority that gave them an edge in task performance. Similarly, in the early kibbutzim, collective villages in Israel that theoretically espoused equality of the sexes but were unable fully to implement it, women could be pushed into traditional service positions (see Tiger and Shepher 1975) because there were often *more than twice as many men as women* in a kibbutz. Again, relative numbers interfered with a fair test of what men or women can "naturally" do (Kanter 1976*b*).

Thus systematic analysis of the dynamics of systems with skewed distributions of social types—tokens in the midst of numerical dominants—is overdue. This paper begins to define a framework for understanding the dynamics of tokenism, illustrated by field observations of female tokens among male dominants.

THE FIELD STUDY

The forms of interaction in the presence of token women were identified in a field study of a large industrial corporation, one of the *Fortune 500* firms (see Kanter [in press] for a description of the setting). The sales force of one division was investigated in detail because women were entering it for the first time. The first saleswoman was hired in 1972; by the end of 1974, there had been about 20 in training or on assignment (several had left the company) out of a sales force of over 300 men. The geographically decentralized nature of sales meant, however, that in training programs or in field offices women were likely to be one of 10 or 12 sales workers; in a few cases, two women were together in a group of a dozen sales personnel. Studying women who were selling industrial goods had particular advantages: (1) sales is a field with strong cultural traditions and folklore and one in which interpersonal skills rather than expertise count heavily, thus making informal and cultural aspects of group interaction salient and visible even for members themselves; and (2) sales workers have to manage relations not only with work peers but

[3] The 17 least active subjects (out of a total of 144) were dropped from the analysis; their sex is not mentioned in published reports. Those 17 might have skewed the sex distribution even further.

with customers as well, thus giving the women two sets of majority groups with which to interact. Sixteen women in sales and distribution were interviewed in depth. Over 40 male peers and managers were also interviewed. Sales-training groups were observed both in session and at informal social gatherings for approximately 100 hours. Additional units of the organization were studied for other research purposes.

THEORETICAL FRAMEWORK

The framework set forth here proceeds from the Simmelian assumption that form determines process, narrowing the universe of interaction possibilities. The form of a group with a skewed distribution of social types generates certain perceptions of the tokens by the dominants. These perceptions determine the interaction dynamics between tokens and dominants and create the pressures dominants impose on tokens. In turn, there are typical token responses to these pressures.

The proportional rarity of tokens is associated with three perceptual phenomena: visibility, polarization, and assimilation. First, tokens, one by one, have higher visibility than dominants looked at alone: they capture a larger awareness share. A group member's awareness share, averaged over shares of other individuals of the same social type, declines as the proportion of total membership occupied by the category increases, because each individual becomes less and less surprising, unique, or noteworthy; in Gestalt terms, they more easily become "ground" rather than "figure." But for tokens there is a "law of increasing returns": as individuals of their type come to represent a *smaller* numerical proportion of the group, they potentially capture a *larger* share of the group members' awareness.

Polarization or exaggeration of differences is the second perceptual tendency. The presence of a person bearing a different set of social characteristics makes members of a numerically dominant group more aware both of their commonalities with and their differences from the token. There is a tendency to exaggerate the extent of the differences, especially because tokens are by definition too few in number to prevent the application of familiar generalizations or stereotypes. It is thus easier for the commonalities of dominants to be defined in contrast to the token than it would be in a more numerically equal situation. One person can also be perceptually isolated and seen as cut off from the group more easily than many, who begin to represent a significant proportion of the group itself.

Assimilation, the third perceptual tendency, involves the use of stereotypes or familiar generalizations about a person's social type. The characteristics of a token tend to be distorted to fit the generalization. If

there are enough people of the token's type to let discrepant examples occur, it is possible that the generalization will change to accommodate the accumulated cases. But if individuals of that type are only a small proportion of the group, it is easier to retain the generalization and distort the perception of the token.

Taylor and Fiske's (1976; Taylor 1975) laboratory experiments provide supportive evidence for these propositions. They played a tape of a group discussion to subjects while showing them pictures of the group and then asked them for their impressions of group members on a number of dimensions. The tape was the same for all subjects, but the purported composition of the group varied. The pictures illustrated either an otherwise all-white male group with one black man (the token condition) or a mixed black-white male group. In the token condition, the subjects paid disproportionate attention to the token, overemphasized his prominence in the group, and exaggerated his attributes. Similarly, the token was perceived as playing special roles in the group, often highly stereotypical ones. By contrast, in the integrated condition, subjects recalled no more about blacks than whites and evaluated their attributes in about the same way.

Visibility, polarization, and assimilation are each associated with particular interaction dynamics that in turn generate typical token responses. These dynamics are similar regardless of the category from which the token comes, although the token's social type and history of relationships with dominants shape the content of specific interactions. Visibility creates performance pressures on the token. Polarization leads to group boundary heightening and isolation of the token. And assimilation results in the token's role entrapment.

PERFORMANCE PRESSURES

The women in the sales force I studied were highly visible, much more so than their male peers. Managers commonly reported that they were the subject of conversation, questioning, gossip, and careful scrutiny Their placements were known and observed throughout the sales unit, while those of men typically were not. Such visibility created a set of performance pressures: characteristics and standards true for tokens alone. Tokens typically perform under conditions different from those of dominants.

1. Public Performance

It was difficult for the women to do anything in training programs or in the field that did not attract public notice. The women found that they

did not have to go out of their way to be noticed or to get the attention of management at sales meetings. One woman reported, "I've been at sales meetings where all the trainees were going up to the managers—'Hi, Mr. So-and-So'—trying to make that impression, wearing a strawberry tie, whatever, something that they could be remembered by. Whereas there were three of us [women] in a group of 50, and all we had to do was walk in, and everyone recognized us."

Automatic notice meant that women could not remain anonymous or hide in the crowd; all their actions were public. Their mistakes and their relationships were known as readily as any other information. It was impossible for them to have any privacy within the company. The women were always viewed by an audience, leading several to complain of "over-observation."

2. Extension of Consequences

The women were visible as category members, and as such their acts tended to have added symbolic consequences. Some women were told that their performance could affect the prospects for other women in the company. They were thus not acting for themselves alone but carrying the burden of representing their category. In informal conversations, they were often measured by two yardsticks: how *as women* they carried out the sales role and how *as salesworkers* they lived up to images of womanhood. In short, every act tended to be evaluated beyond its meaning for the organization and taken as a sign of "how women do in sales." The women were aware of the extra symbolic consequences attached to their acts.

3. Attention to a Token's Discrepant Characteristics

A token's visibility stems from characteristics—attributes of a master status—that threaten to blot out other aspects of the token's performance. While the token captures attention, it is often for discrepant characteristics, for the auxiliary traits that provide token status. No token in the study had to work hard to have her presence noticed, but she did have to work hard to have her achievements noticed. In the sales force, the women found that their technical abilities were likely to be eclipsed by their physical appearance, and thus an additional performance pressure was created. The women had to put in extra effort to make their technical skills known, to work twice as hard to prove their competence. Both male peers and customers would tend to forget information women provided about their experiences and credentials, while noticing and remembering such secondary attributes as style of dress.

4. Fear of Retaliation

The women were also aware of another performance pressure: to avoid making the dominants look bad. Tokenism sets up a dynamic that makes tokens afraid of outstanding performance in group events and tasks. When a token does well enough to show up a dominant, it cannot be kept a secret, because all eyes are on the token. Therefore it is difficult in such a situation to avoid the public humiliation of a dominant. Thus, paradoxically, while the token women felt they had to do better than anyone else in order to be seen as competent and allowed to continue, they also felt in some cases that their successes would not be rewarded and should be kept secret. One woman had trouble understanding this and complained of her treatment by managers. They had fired another woman for not being aggressive enough, she reported; yet she, who succeeded in doing all they asked and had brought in the largest amount of new business during the past year, was criticized for being too aggressive, too much of a hustler.

Responses of Tokens to Performance Pressures

There are two typical ways tokens respond to these performance pressures. The first involves overachievement. Aware of the performance pressures, several of the saleswomen put in extra effort, promoted themselves and their work at every opportunity, and let those around them know how well they were doing. These women evoked threats of retaliation. On the gossip circuit, they were known to be doing well but aspiring too high too fast; a common prediction was that they would be cut down to size soon.

The second response is more common and is typical of findings of other investigators. It involves attempts to limit visibility, to become socially invisible. This strategy characterizes women who try to minimize their sexual attributes so as to blend unnoticeably into the predominant male culture, perhaps by adopting "mannish dress" (Hennig 1970, chap. 6). Or it can include avoidance of public events and occasions for performance—staying away from meetings, working at home rather than in the office, keeping silent at meetings. Several of the saleswomen deliberately kept such a low profile, unlike male peers who tended to seize every opportunity to make themselves noticed. They avoided conflict, risks, and controversial situations. Those women preferring social invisibility also made little attempt to make their achievements publicly known or to get credit for their own contributions to problem solving or other organizational tasks. They are like other women in the research literature who have let others assume visible leadership (Megaree 1969) or take credit

for their accomplishments (Lynch 1973; Cussler 1958). These women did blend into the background, but they also limited recognition of their competence.

This analysis suggests a reexamination of the "fear of success in women" hypothesis. Perhaps what has been called fear of success is really the token woman's fear of visibility. The original research identifying this concept created a hypothetical situation in which a woman was at the top of her class in medical school—a token woman in a male peer group. Such a situation puts pressure on a woman to make herself and her achievements invisible, to deny success. Attempts to replicate the initial findings using settings in which women were not so clearly tokens produced very different results. And in other studies (e.g., Levine and Crumrine 1975), the hypothesis that fear of success is a female-linked trait has not been confirmed. (See Sarason [1973] for a discussion of fear of visibility among minorities.)

BOUNDARY HEIGHTENING

Polarization or exaggeration of the token's attributes in contrast to those of the dominants sets a second set of dynamics in motion. The presence of a token makes dominants more aware of what they have in common at the same time that it threatens that commonality. Indeed it is often at those moments when a collectivity is threatened with change that its culture and bonds become evident to it; only when an obvious outsider appears do group members suddenly realize their common bond as insiders. Dominants thus tend to exaggerate both their commonality and the token's difference, moving to heighten boundaries of which previously they might not even have been aware.[4]

1. Exaggeration of Dominants' Culture

Majority members assert or reclaim group solidarity and reaffirm shared in-group understandings by emphasizing and exaggerating those cultural elements which they share in contrast to the token. The token becomes

[4] This awareness often seemed to be resented by the men interviewed in this study, who expressed a preference for less self-consciousness and less attention to taken-for-granted operating assumptions. They wanted to "get on with business," and questioning definitions of what is "normal" and "appropriate" was seen as a deflection from the task at hand The culture in the managerial/technical ranks of this large corporation, like that in many others, devalued introspection and emphasized rapid communication and ease of interaction. Thus, although group solidarity is often based on the development of strong in-group boundaries (Kanter 1972), the stranger or outsider who makes it necessary for the group to pay attention to its boundaries may be resented not only for being different but also for giving the group extra work.

both occasion and audience for the highlighting and dramatization of those themes that differentiate the token as outsider from the insider. Ironically, tokens (unlike people of their type represented in greater proportion) are thus instruments for under*lining* rather than under*mining* majority culture. In the sales-force case, this phenomenon was most clearly in operation in training programs and at dinner and cocktail parties during meetings. Here the camaraderie of men, as in other work and social settings (Tiger 1969), was based in part on tales of sexual adventures, ability with respect to "hunting" and capturing women, and off-color jokes. Secondary themes involved work prowess and sports. The capacity for and enjoyment of drinking provided the context for displays of these themes. According to male informants' reports, they were dramatized more fervently in the presence of token women than when only men were present. When the men introduced these themes in much milder form and were just as likely to share company gossip or talk of domestic matters (such as a house being built), as to discuss any of the themes mentioned above, this was also in contrast to the situation in more equally mixed male-female groups, in which there were a sufficient number of women to influence and change group culture in such a way that a new hybrid based on shared male-female concerns was introduced. (See Aries [1973] for supportive laboratory evidence.)

In the presence of token women, then, men exaggerated displays of aggression and potency: instances of sexual innuendo, aggressive sexual teasing, and prowess-oriented "war stories." When one or two women were present, the men's behavior involved showing off, telling stories in which masculine prowess accounted for personal, sexual, or business success. The men highlighted what they could do, as men, in contrast to women. In a set of training situations, these themes were even acted out overtly in role plays in which participants were asked to prepare and perform demonstrations of sales situations. In every case involving a woman, men played the primary, effective roles, and women were objects of sexual attention. In one, a woman was introduced as president of a company selling robots; she turned out to be one of the female robots, run by the male company sales manager.

The women themselves reported other examples of testing to see how they would respond to the "male" culture. They said that many sexual innuendos or displays of locker-room humor were put on for their benefit, especially by the younger men. (The older men tended to parade their business successes.) One woman was a team leader and the only woman at a workshop when her team, looking at her for a reaction, decided to use as its slogan "The [obscenity] of the week." By raising the issue and forcing the woman to choose not to participate in the workshop, the

men in the group created an occasion for uniting against the outsider and asserting dominant-group solidarity.

2. Interruptions as Reminders of "Difference"

Members of the numerically dominant category underscore and reinforce differences between tokens and themselves, insuring that the former recognize their outsider status by making the token the occasion for interruptions in the flow of group events. Dominants preface acts with apologies or questions about appropriateness directed at the token; they then invariably go ahead with the act, having placed the token in the position of interrupter or interloper. This happened often in the presence of the saleswomen. Men's questions or apologies were a way of asking whether the old or expected cultural rules were still operative—the words and expressions permitted, the pleasures and forms of release indulged in. (Can we still swear? Toss a football? Use technical jargon? Go drinking? Tell in jokes? See Greenbaum [1971, p. 65] for other examples.) By posing these questions overtly, dominants make their culture clear to tokens and state the terms under which tokens interact with the group.

The answers almost invariably affirm the understandings of the dominants, first because of the power of sheer numbers. An individual rarely feels comfortable preventing a larger number of peers from engaging in an activity they consider normal. Second, the tokens have been put on notice that interaction will not be "natural," that dominants will be holding back unless the tokens agree to acknowledge, permit, and even encourage majority cultural expressions in their presence. (It is important that this be stated, of course, for one never knows that another is holding back unless the other lets a piece of the suppressed material slip out.) At the same time, tokens have also been given the implicit message that majority members do *not* expect those forms of expression to be natural to the tokens' home culture; otherwise majority members would not need to raise the question. (This is a function of what Laws [1975] calls the "double deviance" of tokens: deviant first because they are women in a man's world and second because they aspire inappropriately to the privileges of the dominants.) Thus the saleswomen were often in the odd position of reassuring peers and customers that they could go ahead and do something in the women's presence, such as swearing, that they themselves would not be permitted to do. They listened to dirty jokes, for example, but reported that they would not dare tell one themselves. Via difference-reminding interruptions, then, dominants both affirm their own shared understandings and draw the cultural boundary between themselves and tokens. The tokens learned that they caused interruptions in

"normal" communication and that their appropriate position was more like that of audience than full participant.

3. *Overt Inhibition: Informal Isolation*

In some cases, dominants do not wish to carry out certain activities in the presence of a token; they have secrets to preserve. They thus move the locus of some activities and expressions away from public settings to which tokens have access to more private settings from which they can be excluded. When information potentially embarrassing or damaging to dominants is being exchanged, an outsider audience is not desirable because dominants do not know how far they can trust tokens. As Hughes (1944, 1958) pointed out, colleagues who rely on unspoken understandings may feel uncomfortable in the presence of "odd kinds of fellows" who cannot be trusted to interpret information in just the same way or to engage in the same relationships of trust and reciprocity (see also Lorber 1975). The result is often quarantine—keeping tokens away from some occasions. Thus some topics of discussion were never raised by men in the presence of many of the saleswomen, even though they discussed these topics among themselves: admissions of low commitment to the company or concerns about job performance, ways of getting around formal rules, political plotting for mutual advantage, strategies for impressing certain corporate executives. As researchers have also found in other settings, women did not tend to be included in the networks by which informal socialization occurred and politics behind the formal system were exposed (Wolman and Frank 1975; O'Farrell 1973; Hennig 1970; Epstein 1970). In a few cases, managers even avoided giving women information about their performance as trainees, so that they did not know they were the subject of criticism in the company until they were told to find jobs outside the sales force; those women were simply not part of the informal occasions on which the men discussed their performances with each other. (Several male managers also reported their "fear" of criticizing a woman because of uncertainty about how she would receive it.)

4. *Loyalty Tests*

At the same time that tokens are often kept on the periphery of colleague interaction, they may also be expected to demonstrate loyalty to the dominant group. Failure to do so results in further isolation; signs of loyalty permit the token to come closer and be included in more activities. Through loyalty tests, the group seeks reassurance that tokens will not

turn against them or use any of the information gained through their viewing of the dominants' world to do harm to the group. They get this assurance by asking a token to join or identify with the majority against those others who represent competing membership or reference groups; in short, dominants pressure tokens to turn against members of the latter's own category. If tokens collude, they make themselves psychological hostages of the majority group. For token women, the price of being "one of the boys" is a willingness to turn occasionally against "the girls."

There are two ways by which tokens can demonstrate loyalty and qualify for closer relationships with dominants. First, they can let slide or even participate in statements prejudicial to other members of their category. They can allow themselves to be viewed as exceptions to the general rule that others of their category have a variety of undesirable or unsuitable characteristics Hughes (1944) recognized this as one of the deals token blacks might make for membership in white groups. Saleswomen who did well were told they were exceptions and were not typical women. At meetings and training sessions, women were often the subjects of ridicule or joking remarks about their incompetence. Some women who were insulted by such innuendos found it easier to appear to agree than to start an argument. A few accepted the dominant view fully. One of the first saleswomen denied in interviews having any special problems because she was a woman, calling herself skilled at coping with a man's world, and said the company was right not to hire more women. Women, she said, were unreliable and likely to quit; furthermore, young women might marry men who would not allow them to work. In this case, a token woman was taking over "gate-keeping" functions for dominants (Laws 1975), letting them preserve their illusion of lack of prejudice while she acted to exclude other women.

Tokens can also demonstrate loyalty by allowing themselves and their category to provide a source of humor for the group. Laughing with others, as Coser (1960) indicated, is a sign of a common definition of the situation; to allow oneself or one's kind to be the object of laughter signals a further willingness to accept others' culture on their terms. Just as Hughes (1946, p. 115) found that the initiation of blacks into white groups might involve accepting the role of comic inferior, the saleswomen faced constant pressures to allow jokes at women's expense, to accept kidding from the men around them. When a woman objected, men denied any hostility or unfriendly intention, instead accusing the woman by inference of lacking a sense of humor. In order to cope, one woman reported, "you learn to laugh when they try to insult you with jokes, to let it roll off your back." Tokens thus find themselves colluding with dominants through shared laughter.

Responses of Tokens to Boundary Heightening

Numerical skewing and polarized perceptions leave tokens with little choice about whether to accept the culture of dominants. There are too few other people of the token's kind to generate a counterculture or to develop a shared intergroup culture. Tokens have two general response possibilities. They can accept isolation, remaining an audience for certain expressive acts of dominants, in which case they risk exclusion from occasions on which informal socialization and political activity take place. Or they can try to become insiders, proving their loyalty by defining themselves as exceptions and turning against their own social category.

The occurrence of the second response on the part of tokens suggests a reexamination of the popularized "women-prejudiced-against-women" hypothesis or the "queen bee syndrome" for possible structural (numerical) rather than sexual origins. Not only has this hypothesis not been confirmed in a variety of settings (e.g., Ferber and Huber 1975); but the analysis offered here of the social psychological pressures on tokens to side with the majority also provides a compelling explanation for the kinds of situations most likely to produce this effect, when it does occur.

ROLE ENTRAPMENT

The third set of interaction dynamics centering around tokens stems from the perceptual tendency toward assimilation: the distortion of the characteristics of tokens to fit preexisting generalizations about their category. Stereotypical assumptions and mistaken attributions made about tokens tend to force them into playing limited and caricatured roles in the system.

1. Status Leveling

Tokens are often misperceived initially as a result of their statistical rarity: "statistical discrimination" (U.S. Council of Economic Advisers 1973, p. 106) as distinguished from prejudice. That is, an unusual woman may be treated as though she resembles women on the average. People make judgments about the role played by others on the basis of probabilistic reasoning about the likelihood of what a particular kind of person does. Thus the saleswomen like other tokens encountered many instances of mistaken identity. In the office, they were often taken for secretaries; on the road, especially when they traveled with male colleagues, they were often taken for wives or mistresses; with customers, they were usually assumed to be substituting for men or, when with a male peer, to be assistants; when entertaining customers, they were assumed to be wives or dates.

Such mistaken first impressions can be corrected. They require tokens to spend time untangling awkward exchanges and establishing accurate and appropriate role relations, but they do permit status leveling to occur. Status leveling involves making adjustments in perception of the token's professional role to fit the expected position of the token's category—that is, bringing situational status in line with master status, the token's social type. Even when others knew that the token saleswomen were not secretaries, for example, there was still a tendency to treat them like secretaries or to make demands of them appropriate to secretaries. In the most blatant case, a woman was a sales trainee along with three men; all four were to be given positions as summer replacements. The men were all assigned to replace salesmen; the woman was asked to replace a secretary—and only after a long, heated discussion with the manager was she given a more professional assignment. Similarly, when having professional contacts with customers and managers, the women felt themselves to be treated in more wifelike or datelike ways than a man would be treated by another man, even though the situation was clearly professional. It was easier for others to make their perception of the token women fit their preexisting generalizations about women than to change the category; numerical rarity provided too few examples to contradict the generalization. Instances of status leveling have also been noted with regard to other kinds of tokens such as male nurses (Segal 1962); in the case of tokens whose master status is higher than their situational status, leveling can work to their advantage, as when male nurses are called "Dr."

2. Stereotyped Role Induction

The dominant group can incorporate tokens and still preserve their generalizations about the tokens' kind by inducting them into stereotypical roles; these roles preserve the familiar form of interaction between the kinds of people represented by the token and the dominants. In the case of token women in the sales force, four role traps were observed, all of which encapsulated the women in a category the men could respond to and understand. Each centered on one behavioral tendency of the token, building upon this tendency an image of her place in the group and forcing her to continue to live up to the image; each defined for dominants a single response to her sexuality. Two of the roles are classics in Freudian theory: the mother and the seductress. Freud wrote of the need of men to handle women's sexuality by envisioning them as either madonnas or whores—as either asexual mothers or overly sexual, debased seductresses. (This was perhaps a function of Victorian family patterns, which encouraged separation of idealistic adoration of the mother and animalistic eroticism [Rieff 1963; Strong 1973].) The other roles, termed

the pet and the iron maiden, also have family counterparts in the kid sister and the virgin aunt.

A. Mother.—A token woman sometimes finds that she has become a mother to a group of men. They bring her their troubles, and she comforts them. The assumption that women are sympathetic, good listeners, and can be talked to about one's problems is common in male-dominated organizations. One saleswoman was constantly approached by her all-male peers to listen to their domestic problems. In a variety of residential-sales-training groups, token women were observed acting out other parts of the traditional nurturant-maternal role: cooking for men, doing their laundry, sewing on buttons.

The mother role is a comparatively safe one. She is not necessarily vulnerable to sexual pursuit (for Freud it was the very idealization of the madonna that was in part responsible for men's ambivalence toward women), nor do men need to compete for her favors, because these are available to everyone. However, the typecasting of women as nurturers has three negative consequences for a woman's task performance: (1) the mother is rewarded by her male colleagues primarily for service to them and not for independent action. (2) The mother is expected to keep her place as a noncritical, accepting, good mother or lose her rewards because the dominant, powerful aspects of the maternal image may be feared by men. Since the ability to differentiate and be critical is often an indicator of competence in work groups, the mother is prohibited from exhibiting this skill. (3) The mother becomes an emotional specialist. This provides her with a place in the life of the group and its members. Yet at the same time, one of the traditionally feminine characteristics men in positions of authority in industry most often criticize in women (see Lynch 1973) is excess emotionality. Although the mother herself might not ever indulge in emotional outbursts in the group, she remains identified with emotional matters. As long as she is in the minority, it is unlikely that nurturance, support, and expressivity will be valued or that a mother can demonstrate and be rewarded for critical, independent, task-oriented behaviors.

B. Seductress.—The role of seductress or sexual object is fraught with more tension than the maternal role, for it introduces an element of sexual competition and jealousy. The mother can have many sons; it is more difficult for a sex object to have many lovers. Should a woman cast as sex object, that is, seen as sexually desirable and potentially available ("seductress" is a perception, and the woman herself may not be consciously behaving seductively), share her attention widely, she risks the debasement of the whore. Yet should she form a close alliance with any man in particular, she arouses resentment, particularly because she represents a scarce resource; there are just not enough women to go around.

In several situations observed, a high-status male allied himself with a seductress and acted as her "protector," not only because of his promise to rescue her from the sex-charged overtures of the rest of the men but also because of his high status per se. The powerful male (staff member, manager, sponsor, etc) can easily become the protector of the still "virgin" seductress, gaining through masking his own sexual interest what other men could not gain by declaring theirs. However, the removal of the seductress from the sexual marketplace contains its own problems. Other men may resent a high-status male for winning the prize and resent the woman for her ability to get an in with the high-status male that they themselves could not obtain as men. While the seductress is rewarded for her femaleness and insured attention from the group, then, she is also the source of considerable tension; and needless to say, her perceived sexuality blots out all other characteristics.

Men may adopt the role of protector toward an attractive woman, regardless of her collusion, and by implication cast her as a sex object, reminding her and the rest of the group of her sexual status. In the guise of helping her, protectors may actually put up further barriers to a solitary woman's full acceptance by inserting themselves, figuratively speaking, between the woman and the rest of a group. A male sales trainer typically offered token women in training groups extra help and sympathetically attended to the problems their male peers might cause, taking them out alone for drinks at the end of daily sessions.

C. Pet.—The pet is adopted by the male group as a cute, amusing little thing and taken along on group events as symbolic mascot—a cheerleader for the shows of male prowess that follow. Humor is often a characteristic of the pet. She is expected to admire the male displays but not to enter into them; she cheers from the sidelines. Shows of competence on her part are treated as extraordinary and complimented just because they are unexpected (and the compliments themselves can be seen as reminders of the expected rarity of such behavior). One woman reported that, when she was alone in a group of men and spoke at length on an issue, comments to her by men after the meeting often referred to her speech-making ability rather than to what she said (e.g., "You talk so fluently"), whereas comments the men made to one another were almost invariably content or issue oriented. Competent acts that were taken for granted when performed by males were often unduly fussed over when performed by saleswomen, who were considered precocious or precious at such times. Such attitudes on the part of men in a group encourage self-effacing, girlish responses on the part of solitary women (who after all may be genuinely relieved to be included) and prevent them from realizing or demonstrating their own power and competence.

D. Iron maiden.—The iron maiden is a contemporary variation of the

stereotypical roles into which strong women are placed. Women who fail to fall into any of the first three roles and in fact resist overtures that would trap them in such roles (like flirtation) might consequently be responded to as though tough or dangerous. (One saleswoman developed just such a reputation in company branches throughout the country.) If a token insisted on full rights in the group, if she displayed competence in a forthright manner, or if she cut off sexual innuendos, she was typically asked, "You're not one of those women's libbers, are you?" Regardless of the answer, she was henceforth viewed with suspicion, treated with undue and exaggerated politeness (by references to women inserted into conversations, by elaborate rituals of *not* opening doors), and kept at a distance; for she was demanding treatment as an equal in a setting in which no person of her kind had previously been an equal. Women inducted into the iron maiden role are stereotyped as tougher than they are (hence the name) and trapped in a more militant stance than they might otherwise take.

Responses of Tokens to Role Entrapment

The dynamics of role entrapment tend to lead to a variety of conservative and low-risk responses on the part of tokens. The time and awkwardness involved in correcting mistaken impressions often lead them to a preference for already-established relationships, for minimizing change and stranger contact in the work situation. It is also often easier to accept stereotyped roles than to fight them, even if their acceptance means limiting a token's range of expressions or demonstrations of task competence, because acceptance offers a comfortable and certain position. The personal consequence for tokens, of course, is a certain degree of self-distortion. Athanassiades (1974), though not taking into account the effects of numerical representation, found that women, especially those with low risk-taking propensity, tended to distort upward communication more than men and argued that many observed work behaviors of women may be the result of such distortion and acceptance of organizational images. Submissiveness, frivolity, or other attributes may be feigned by people who feel these are prescribed for them by the dominant organizational culture. This suggests that accurate conclusions about work attitudes and behavior cannot be reached by studying people in the token position, since there may always be an element of compensation or distortion involved. Thus many studies of professional and managerial women should be reexamined in order to remove the effects of numbers from the effects of sex roles.

IMPLICATIONS

This paper has developed a framework for understanding the social perceptions and interaction dynamics that center on tokens, using the example of women in an industrial sales force dominated numerically by men. Visibility generates performance pressures, polarization generates group-boundary heightening, and assimilation generates role entrapment. All of the phenomena associated with tokens are exaggerated ones: the token stands out vividly, group culture is dramatized, boundaries become highlighted, and token roles are larger-than-life caricatures.

The concepts identified here are also applicable to other kinds of tokens who face similar interaction contexts. Hughes's (1944, 1946, 1958) discussions of the problems encountered by blacks in white male work groups are highly congruent with the framework presented here. Taylor and Fiske's (1976) laboratory research demonstrates the perceptual phenomena mentioned above in the black-white context. Segal (1962) also provides confirming evidence that, when men are tokens in a group of women, the same concepts apply. He studied a hospital in which 22 out of 101 nurses were men. He found that male nurses were isolates in the hospital social structure, not because the men disassociated themselves from their women peers but because the women felt the men were out of place and should not be nurses. Male and female nurses had the same objective rank, but people of both sexes felt that the men's subjective status was a lower one. The women placed the men in stereotypical positions, expecting them to do the jobs the women found distasteful or considered men's work. During a personal interview, a male nursing student reported that he thought he would enjoy being the only man in a group of women until he found that he engendered a great deal of hostility and that he was teased every time he failed to live up to the manly image, for example, if he was vague or subjective in speech. And "token men" working in child-care centers were found to play minor roles, become social isolates, and bear special burdens in interaction, which they handled like the saleswomen, by defining themselves as "exceptional" men (Seifert 1974). Similarly, a blind informant indicated to me that, when he was the only blind person among sighted people, he often felt conspicuous and more attended to than he liked. This in turn created pressure for him to work harder in order to prove himself. In the solo situation, he was never sure that he was getting the same treatment as other members of the group (first, fellow students; later, fellow members of an academic institution), and he suspected that people tended to protect him. When he was the only one of his kind, as opposed to situations in which other blind people were present, sighted people felt free to grab his arm and

pull him along and were more likely to apologize for references to visual matters, reinforcing his sense of being different and cast in the role of someone more helpless than he in fact perceived himself to be.

If the token's master status is higher than that of the situational dominants, some of the content of the interaction may change while the dynamics remain the same. A high-status token, for example, might find that the difference-reminding interruptions involve deference and opinion seeking rather than patronizing apology; a high-status token might be allowed to dominate formal colleague discussion while still being excluded from informal, expressive occasions. Such a token might be trapped in roles that distort competence in a favorable rather than an unfavorable direction; but distortion is involved nonetheless. Further research can uncover appropriate modifications of the framework which will allow its complete extension to cases in the category just discussed.

The analysis undertaken here also suggests the importance of intermediate-level structural and social psychological variables in affecting male-female interaction and the roles of women in work groups and organizations. Some phenomena that have been labeled sex related but have not been replicated under all circumstances might be responses to tokenism, that is, reflections of responses to situational pressures rather than to sex differences. "Fear of success" might be more fruitfully viewed as the fear of visibility of members of minority groups in token statuses. The modesty and lack of self-aggrandizement characteristic of some professional and managerial women might be accounted for in similar ways, as situational responses rather than sex-linked traits. The prejudice of some women against others might be placed in the context of majority-culture loyalty tests. The unwillingness of some professional and managerial women to take certain risks involving a change in relationships might be explained as a reasonable response to the length of time it may take a token to establish competence-based working relationships and to the ever-present threat of mistaken identity in new relationships.

The examination of numerical effects leads to the additional question of tipping points: how many of a category are enough to change a person's status from token to full group member? When does a group move from skewed to tilted to balanced? Quantitative analyses are called for in order to provide precise documentation of the points at which interaction shifts because enough people of the "other kind" have become members of a group. This is especially relevant to research on school desegregation and its effects or changing neighborhood composition as well as occupational segregation by sex. Howe and Widick (1949, pp. 211–12) found that industrial plants with a small proportion of blacks in their work force had racial clashes, whereas those plants in which blacks constituted a large proportion had good race relations.

Exact tipping points should be investigated. Observations from the present study make it clear that even in small groups two of a kind are *not* enough. Data were collected in several situations in which two women rather than one were found among male peers but still constituted less than 20% of the group. Despite Asch's (1960) laboratory finding that one potential ally is enough to reduce the power of the majority to secure conformity, in the two-token situation in organizations dominants were nearly always able to defeat an alliance between two women by setting up invidious comparisons. By the exaggeration of traits in both cases, one woman was identified as a success, the other as a failure. The one given the positive label felt relieved to be accepted and praised. She recognized that alliance with the identified failure would jeopardize her acceptance. The consequence in one sales office was that the identified success stayed away from the other woman, did not give her any help with her performance, and withheld criticism she had heard that might have been useful. The second woman soon left the organization. In another case, dominants defeated an alliance, paradoxically by trying to promote it Two women in a training group of 12 were treated as though they were an automatic pair, and other group members felt that they were relieved of responsibility for interacting with or supporting the women. The women reacted to this forced pairing by trying to create differences between themselves and becoming extremely competitive. Thus structural circumstances and pressures from the majority can produce what appear to be prejudicial responses of women to each other. Yet these responses are best seen as the effects of limited numbers Two (or less than 20% in any particular situation) is not always a large enough number to overcome the problems of tokenism and develop supportive alliances, unless the tokens are highly identified with their own social category.

Tokens appear to operate under a number of handicaps in work settings. Their possible social isolation may exclude them from situations in which important learning about a task is taking place and may also prevent them from being in a position to look good in the organization. Performance pressures make it more dangerous for tokens to fumble and thus give them less room for error. Responding to their position, they often either underachieve or overachieve, and they are likely to accept distorting roles which permit them to disclose only limited parts of themselves. For all these reasons, in situations like industrial sales in which informal interaction provides a key to success tokens are not very likely to do well compared with members of the majority category, at least while in the token position.

These consequences of token status also indicate that tokens may undergo a great deal of personal stress and may need to expend extra energy to maintain a satisfactory relationship in the work situation. This fact is

reflected in their common statements that they must work twice as hard as dominants or spend more time resolving problematic interactions. They face partially conflicting and often completely contradictory expectations. Such a situation has been found to be a source of mental stress for people with inconsistent statuses and in some cases to reinforce punitive self-images. In addition, turning against others of one's kind may be intimately connected with self-hatred. Finally, tokens must inhibit some forms of self-expression and often are unable to join the group in its characteristic form of tension release. They may be asked to side with the group in its assaults-through-humor but often cannot easily join the group in its play. They potentially face the stresses of social isolation and self-distortion.[5]

Thus social-policy formulations might consider the effects of proportions in understanding the sources of behavior, causes of stress, and possibilities for change. The analysis of tokenism suggests, for example, that merely adding a few women at a time to an organization is likely to give rise to the consequences of token status. Despite the contemporary controversy over affirmative action quotas (Glazer 1976), numbers do appear to be important in shaping outcomes for disadvantaged individuals. Women (or members of any other underrepresented category) need to be added to total group or organization membership in sufficient proportion to counteract the effects of tokenism. Even if tokens do well, they do so at a cost, overcoming social handicaps, expending extra effort, and facing stresses not present for members of the numerically dominant group. The dynamics of tokenism also operate in such a way as to perpetuate the system that keeps members of the token's category in short supply; the presence of a few tokens does not necessarily pave the way for others—in many cases, it has the opposite effect.

Investigation of the effects of proportions on group life and social interaction appears to be fruitful both for social psychological theory and for understanding male-female interaction. It is a step toward identifying the structural and situational variables that intervene between global cultural definitions of social type and individual responses—that shape the context for face-to-face interactions among different kinds of people. Relative as well as absolute numbers can be important for social life and social relations.

[5] The argument that tokens face more personal stress than majority group members can be supported by studies of the psychosocial difficulties confronting people with inconsistent statuses Among the stresses identified in the literature on class and race are unsatisfactory social relationships, unstable self-images, frustration over rewards, and social ambiguity (Hughes 1944, 1958; Lenski 1956; Fenchel, Monderer, and Hartley 1951, Jackson 1962). Token women must also inhibit self-expression and self-disclosure, as the examples in this paper and the discussion below indicate; yet Jourard (1964) considers the ability to self-disclose a requisite for psychological well-being.

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Ending the Vietnam War: Components of Change in Senate Voting on Vietnam War Bills[1]

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Very little quantitative academic work has dealt with the politics of American involvement in and withdrawal from Indochina. This article is a preliminary examination of how the U.S. Senate moved from a strong pro-involvement stance to a strong anti-involvement one. The main findings are: (1) the aggregate change in Senate voting came about disproportionately through replacement of supporters of the war by opponents, as opposed to changes of mind by incumbents; (2) nevertheless, dovish bills adopted toward the end of the conflict would have passed even without the support of replacements, because incumbents were converting fairly rapidly; (3) doves were disproportionately Democrats, relatively young, low in seniority, and from the northeast and north central states; (4) elections were important in the Senate change of mind, but often not in the way expected; rather than doves defeating hawks toward the end of the war, hawks tended disproportionately to die or retire and may have been replaced by doves because most candidates running by the end of the war were doves. Theoretical implications and research proposals are discussed.

What caused the end of direct American involvement in the Vietnam War? A list of causes could be quite long; it would include military reverses, changes in public opinion, changes in media coverage, and so on. Ultimately, however, the various factors were important only insofar as they affected the decisions of Congress and the president: American involvement ended after Congress "changed its mind" and cut off all further funding for the war. This paper is a partial and preliminary attempt to analyze that change in the Senate.

In 1964, the Senate passed the so-called Gulf of Tonkin resolution (House Joint Resolution 1145) by a vote of 88 to 2; the resolution was interpreted by President Johnson as approving massive American involvement in Vietnam, and senators implicitly agreed with that interpretation, as they passed bills financing the involvement by similarly overwhelming majorities. But less than 10 years later the Senate passed a supplemental appropriations bill (HR 9055, 1973) which prohibited any expenditure of

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funds for American military involvement in the Indochina war, and so ended American involvement.

Obviously a massive shift in opinion with important consequences had taken place in the meantime. Yet very little academic work has been done on the shift, and most of what exists concentrates on public opinion or the interplay of personalities.[2] In contrast, this article deals directly with the Senate's change of mind; it focuses on the changes in aggregate outcomes—roll call votes—that had a direct impact on American participation in the war.

At this level, there were two processes that brought about the shift in opinion: some prowar senators "converted" to antiwar positions, and others were replaced by new senators who opposed the war.[3] In this paper we call these processes "conversion" and "replacement," and we begin to deal with three questions about them:

1. What was the relative importance of replacement as opposed to conversion in the Senate's change of mind? Answering this question is important for two major reasons. First, knowledge of the relative importance of replacement and conversion will give us some indication of how political change takes place: If conversion is important, it is at least possible that representatives are responsive to changes in public opinion; if it is not, we might infer that legislative change comes about generally by election of new representatives.[4] Second and more generally, assessing the importance of conversion and replacement involves broader questions about social change. Some writers have proposed that those already committed to positions are relatively unlikely to change their opinions; consequently, changes in mass behavior, legislative change, and the acceptance of new ideas by groups are likely to depend disproportionately upon the exit of old members committed to old ideas and their replacement by new members (see, e.g., Ryder 1965; Converse 1969; Gusfield 1957; Bullock 1972; Kuhn 1968). We will test this hypothesis with regard to Senate voting on the Vietnam issue.

2. What sorts of senators were especially likely to become doves, and what sorts of new senators were especially likely to be doves? Specifically, was dovishness correlated with political party, region, age, or seniority?

[2] Most analyses by historians, political scientists, and journalists have been discursive narratives. The few analytical quantitative studies tend to focus on public opinion on the war, rather than on government action (see, e g , Page and Brody 1972; Verba and Brody 1970; Converse et al 1969). There are two attempts to predict aggregate public opinion from the war situation, but they do not deal with congressional voting (see Milstein 1973; Mueller 1973; cf. Sullivan and O'Connor 1972).

[3] Movement from opposition to support is possible, of course, and happened occasionally, but the bulk of movement was from support to opposition.

[4] Representatives can also change their minds independently of constituents' desires, of course.

Examining the correlates of conversion and replacement will help show us what types of persons and groups were disproportionately responsible for the change in policy.

3. What was the relationship between the shift in senate voting and the electoral process; for example, did the public express its preferences by defeating hawks disproportionately and replacing them with doves? We will begin trying to discover how and in what ways changes in senate roll call outcomes are related to election results.

DATA

Between the passage of the Gulf of Tonkin resolution in 1964 and the fund cutoff in 1973, Congress voted many times on Vietnam-related bills, most often on appropriations bills. This paper will focus on five roll calls which marked particularly important points in the Senate change of mind.[5]

The first is the passage of the Southeast Asia resolution (the Gulf of Tonkin resolution) of 1964 (House Joint Resolution 1145, Public Law 88-408), which marks the beginning of large-scale American involvement. Because the Senate's vote (*Congressional Quarterly Almanac* [*CQ*], Roll Call 252, 1964) for the resolution was 88 to 2, this roll call can provide a baseline measure of Senate personnel who had to convert or be replaced before the aggregate Senate outcome could change, that is, it is not a measure of disagreement over policy.

The second roll call in question is a proposal by Senator Proxmire to limit expenditures on bombing (*CQ*, Roll Call 163, 1968). Not considered significant at the time, the proposal lost, 10 to 79, but it was the first "dovish" proposal to gain even 10 supporters; those senators can be seen as an initial core group of doves.[6]

The third and fourth roll calls took place in 1970. The Cooper-Church amendment prohibiting the use of funds for American ground troops and advisers in Cambodia passed 58 to 32 (*CQ*, Roll Call 180), but the McGovern-Hatfield amendment limiting the number of troops in Vietnam

[5] Odd as it may seem to those unfamiliar with Congress, a roll call vote was never taken on the issue of cutting off combat in Vietnam specifically, or in Southeast Asia as a whole. Roll Call 147, the last of the five to be considered, referred only to Cambodia and Laos; the bill of which it was a part passed the Senate 73 to 5, and was vetoed by the president. In the bill intended to replace HR 7447 after it was vetoed, an amendment referring to Vietnam was added by voice vote in the House, and the Senate adopted the House bill, which was by this time acceptable to the president. The final bill, HR 9055, passed almost without formal opposition, but the vote on it would be a poor indicator of individual or collective opinion on the war; for that purpose, students of Congress would find the 55 to 21 vote the most suitable.

[6] Note that despite the public turmoil over Vietnam that had taken place by 1968 and despite President Johnson's decision not to seek reelection, etc., the Senate passed no significant antiwar measure until 1970.

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and setting a withdrawal date was defeated 39 to 55 (*CQ*, Roll Call 258). By this time, Senate opinion had changed to the point where the senators were willing to end involvement in Cambodia, but not in Vietnam.

The fifth and final roll call to be considered is the one which comes closest to being the vote which cut off all funds for further southeast Asian combat; that was a 55 to 21 roll call on an amendment to a 1973 supplemental appropriations bill, HR 7447 (*CQ*, Roll Call 147).[7]

FINDINGS

Conversion and Replacement

Tables 1 and 2 allow us to answer three questions posed here about conversion and replacement. First, many students of social change (as noted above) hypothesize that "new blood" is required before an organization can undergo significant change; is the *rate of change* from nondove to dove[8] faster through replacement than through conversion, as such a position would suggest? Second, when will the *total number* of doves who are replacements exceed the number who are converts?[9] Third, is it possible that differences between senators who remain in office and new senators could have *affected the actual outcome of a roll call;* that is, can we say that a winning (or losing) proposal would have lost (or won) had no senators been replaced since the previous roll call? In other words, did changes in Senate personnel actually affect policy outcomes?

Table 1 is set up as follows: The first column is the net gain in the num-

[7] Readers have raised two points about the treatment of the five roll calls First, the dove-hawk dichotomy may be too simple, more sophisticated scaling might be appropriate The second and related point concerns the subjective meaning of the votes for the senators, although the analysis implicitly arrays the roll calls along a single hawk-dove dimension, the bills and amendments might not have been perceived by the actors as forming such a dimension The points have merit From a practical standpoint, however, we feel that the methods we use are adequate in a preliminary analysis such as this In addition, because we are interested in aggregate outcomes, the subjective perceptions of individual senators are irrelevant here we are concerned, not with why individual senators voted as they did, but with why policy changed over time We should add, however, that subsequent work we have done on Senate voting on all Vietnam-related bills indicates that the voting can be described satisfactorily in terms of three dimensions which had continuous importance throughout the war, although the position of the Senate changed, the dimensions of conflict remained stable for the duration of the war (Burstein 1976).

[8] The analysis is of the shift from nondove to dove, rather than from hawk to dove, so that all those voting dove on the second roll call can be accounted for, including those who did not vote on the first roll call The following votes are considered dovish those *against* the Gulf of Tonkin resolution, those *for* all the other proposals

[9] Some of the nondove senators who left office might have converted had they remained in the Senate. There is no truly satisfactory way to deal with this problem, but an attempt is made below.

TABLE 1

RATE OF CHANGE FROM NONDOVE TO DOVE THROUGH CONVERSION AND REPLACEMENT, BETWEEN PAIRS OF ROLL CALLS

PAIRS OF ROLL CALLS	NET* CHANGE FROM NONDOVE TO DOVE	CONVERTS, AS PROPORTION OF THOSE AVAILABLE†		REPLACEMENTS, AS PROPORTION OF THOSE AVAILABLE‡	
		N	%	*N*	%
Tonkin to:					
Proxmire	8	7/82	8	1/16	7
Cooper	56	37/69	54	19/29	65
McGovern	37	26/67	38	11/29	38
Cutoff	53	26/51	51	27/47	57
Proxmire to:					
Cooper	48	38/75	51	10/14	71
McGovern	29	24/75	32	5/14	36
Cutoff	45	23/53	43	22/36	61
Cooper to:					
McGovern§	−16	−16/58	−28	...	...
Cutoff	− 3	− 7/29	−24	4/13	31
McGovern to:					
Cutoff	16	6/42	14	10/19	53

NOTE.—Tonkin = Gulf of Tonkin resolution, RC 252, 1964, Proxmire = proposal to limit bombing, RC 163, 1968, Cooper = Cooper-Church amendment, RC 180, 1970, McGovern = McGovern-Hatfield amendment, RC 258, 1970, Cutoff = final fund cutoff, RC 147, 1973

* (new doves) − (new nondoves) = net gain in number of doves, comparing the second roll call listed to the first

† Converts = net gain in number of doves through conversion, i.e., from among those in Congress for both votes (converts from nondove to dove) − (converts from dove to nondove). Those available = those nondove on first roll call, of those in Congress for both votes

‡ Replacements = net gain in number of doves through replacement, i.e., from among seats whose incumbents were replaced between roll calls (senators who entered after first roll call who are doves) − (doves who left before second roll call). Those available = those nondove on first roll call, of seats changing hands

§ Based on shifts from dove to hawk. Because both roll calls were in the same session, there were no replacements. Negative numbers mean shift away from dovishness

ber of doves between votes. Columns 2 and 3 divide that net gain into its "conversion" and "replacement" components, respectively; for purposes of comparison, both sets of figures are expressed as proportions of those who could conceivably have *become* doves, namely, previous *non*doves—ones still in Congress and thus potential converts (col. 2), and ones who left Congress for replacements (col. 3). Since the numerators show the net gain in the number of doves (by whichever mechanism) between votes, the fractions show the proportion of potential new doves who actually became doves.[10]

The table shows that, while both processes occurred at fairly similar rates initially, replacement is generally associated with a faster rate of change from nondove to dove than conversion is, and that the differences become striking by the time of the final cutoff. Replacement senators were more

[10] The numerator is the net, not total, gain in doves; but even though some former doves could have become hawks, the numbers are very small in all the votes but one (see table 2), and do not distort the conclusions.

TABLE 2

INCREASE IN NUMBER OF DOVES DUE TO CONVERSION AND REPLACEMENT

Pairs of Roll Calls	N Doves, First Roll Call	Gain, for Second Roll Call, from: Converts	Replacements	N Doves, Second Roll Call	Hypothetical Total*
Tonkin to:					
Proxmire	2	7	1	10	10
Cooper	2	37	19	58	55
McGovern	2	26	11	39	39
Cutoff	2	26	27	55	52
Proxmire to:					
Cooper	10	38	10	58	55
McGovern	10	24	5	39	38
Cutoff	10	23	22	55†	48
Cooper to:					
McGovern‡	58	−16	...	39	...
Cutoff	58	− 7	4	55†	48
McGovern to:					
Cutoff	39	6	10	55†	48

* Hypothetical total if replacement rate = conversion rate, total number of doves on second vote if the senators who were replaced had not been—i e , had stayed in the Senate—and had converted at the same rate as those who actually stayed

† Cases where winning proposal could have been defeated were assumption true.

‡ Dove to hawk, negative because conversion was away from dovishness

likely to vote for the fund cutoff than were those who had been in office continuously.[11] Although a substantial proportion of those available did convert, the data support the view that change in the balance of opinion depends disproportionately upon the replacement of personnel holding one view by new personnel holding another view.

In terms of actual roll call outcomes, however, the importance of a group depends upon its relative size as well as its relative dovishness. If the replacement rate is very slow, replacements will have an impact on roll call outcomes only where the votes are close or the time span is long. Electoral laws set a maximum rate of replacement in the senate—one-third of senators every two years—but do not set a minimum rate. In fact, 49 of the senators in office at the time of the Gulf of Tonkin resolution had been replaced by the 1973 fund cutoff vote. Table 1 shows that there were more replacement doves than converted ones in three of the 10 comparisons between roll calls: between Tonkin and the cutoff, Cooper-Church and the cutoff, and McGovern-Hatfield and the cutoff. In addition, of the increase of 45 in the number of doves between the Proxmire and cutoff votes, 22—almost half—were replacements. Thus, by the end of active American involvement, the senators who had taken office since the Gulf of Tonkin

[11] No tests of significance are presented because the data describe a universe, not a sample.

resolution were a very substantial proportion of the majority which voted in favor of the final fund cutoff. Replacement doves were numerically as well as disproportionately important among the senators who helped end the war.

The question whether changes in Senate personnel actually affected any of the roll call outcomes is impossible to answer with certainty, because we do not know how those who left would have voted had they stayed. Table 2 provides a basis for informed speculation, however. The figures in column 5 show the number of dovish votes each roll call would have received if the entire Senate had converted from nondove to dove at the same rate as those senators who were not replaced between roll calls; that is, it was assumed, contrary to the fact, that replacement was associated with the same rate of change to dovishness as conversion was. These hypothetical figures lead to two conclusions: First, although the number of doves on most roll calls would have been lower than it actually was, in no case would the "expected" number have been less than a majority of those voting on the roll call. For example, of the 76 voting on the fund cutoff, 55 actually voted for it; 48 would be expected to have done so if the assumption held—fewer than 55 but more than the 39 required to pass the proposal. Thus, while replacement was a more effective mechanism for change than conversion, it could be claimed that (on these roll calls) the results might have been the same had no replacement taken place.

Second, however, the hypothetical shift from 55 actual dovish votes to 48 under the assumption of no difference between incumbents and replacements does reduce the number of votes for a cutoff from an absolute majority of the Senate to a minority. It is extremely unlikely that all those who did not vote on the cutoff were against it, but if they had been, the difference between incumbents and replacements could conceivably have affected the outcome.[12]

Differences between incumbents and replacements cannot be said to have directly affected any roll call outcomes. This is true at least in part because those remaining in Congress were converting to dovishness at a fairly rapid rate; it is likely that both newcomers and incumbents were voting as doves because both were responding to changes in public opinion, the war situation, etc. Replacements were disproportionately likely to be doves (as compared to those who remained in the Senate), especially toward the end of the war; in addition, they were a numerically important part of the dovish vote by 1973. However, the importance of conversion underscores the fact that it is possible to change policies without changing personnel; in cases

[12] Concern with aggregate voting outcomes is unusual in studies of voting in mass publics and in legislatures, yet this neglect is difficult to understand· presumably elections and roll calls are important objects of study because of the importance of the outcomes.

such as the present one, changes may in fact be initiated by incumbents, with replacement functioning to solidify those changes once they are begun, removing some portion of the "hard core" opponents.

Dovishness in Relation to Party, Age, Seniority, and Region

This section deals with three questions. Was dovishness related to the party, age, seniority, and region of senators? Did these four factors engender differing propensities to move from a nondove position to a dovish position, that is, did certain groups lead the Senate change of mind and other groups follow? Finally, was the process of change the same in all groups, or, for example, did the Senators of one region become doves by conversion while those of another changed through replacement?

Tables 3 and 4 present the results for party and age; the results for seniority and region, being simple, need not be presented in tables.

TABLE 3

NET CHANGE FROM NONDOVE TO DOVE, BY PARTY, AS A PROPORTION OF THOSE AVAILABLE, FOR CONVERTS AND REPLACEMENTS

	Converts				Replacements			
	Democrats		Republicans		Democrats		Republicans	
Pairs of Roll Calls	*N*	%	*N*	%	*N*	%	*N*	%
Tonkin to:								
Proxmire	6/55	11	1/27	4	0/10	0	1/6	17
Cooper	32/46	70	5/23	22	8/19	42	11/10	110*
McGovern	24/46	52	2/23	9	6/19	32	5/10	50
Cutoff	22/37	59	4/14	29	13/28	46	14/19	74
Proxmire to:								
Cooper	32/47	68	6/28	22	2/9	22	8/6	133*
McGovern	22/47	47	2/28	7	2/9	22	3/6	50
Cutoff	18/35	51	5/18	28	11/21	52	11/15	74
Cooper to:								
McGovern†	−8/42‡	−19	−8/16	−50	...	...	...	...
Cutoff	−6/12	−50	−1/17	− 6	3/4	75	3/9	33
McGovern to:								
Cutoff	0/18	0	6/24	25	1/5	20	5/11	45

NOTE Explanations for table 1 apply to all tables In this table, those available are calculated as in table 1 but within parties

* Percentages greater than 100 are made possible by a changing party balance in the Senate Between the Tonkin and Cooper roll calls, 10 Republicans who had been nondoves on the Tonkin roll call left the Senate, but on the Cooper roll call, there were 11 Republican doves who had not been in Congress for the Tonkin roll call

† Dove to hawk

‡ Negative numbers mean shift was away from dovishness

Table 3 presents net changes in dovishness, by party, between pairs of votes. The numerators of the fractions show, to no one's surprise, that most

TABLE 4

AGE AND ROLL CALL VOTING. MEAN AGE OF THOSE VOTING IN SPECIFIED PATTERNS

Pairs of Roll Calls	Voting: Dove to Dove	Dove to Nondove	Nondove to Dove	Nondove to Nondove
Tonkin to·				
Proxmire	.	..	65 (8)	69 (74)
Cooper	.	. .	65 (37)	70 (32)
McGovern	.	..	63 (26)	70 (43)
Cutoff	..	..	65 (26)	65 (25)
Proxmire to:				
Cooper	63 (7)	.	62 (39)	69 (36)
McGovern	63 (8)	.	60 (24)	68 (51)
Cutoff	60 (5)	...	64 (25)	64 (28)
Cooper to·				
McGovern*	59 (42)	63 (16)	...	68 (39)
Cutoff	59 (34)	58 (12)	67 (5)	65 (24)
McGovern to:				
Cutoff	59 (27)	56 (6)	63 (12)	64 (30)
	Dove to Not in Congress	**Nondove to Not in Congress**	**Not in Congress Dove**	**Not in Congress Nondove**
Tonkin to:				
Proxmire	...	74 (16)	...	56 (14)
Cooper	..	74 (29)	52 (21)	59 (10)
McGovern	...	74 (29)	51 (13)	57 (18)
Cutoff	...	74 (47)	50 (29)	54 (20)
Proxmire to.				
Cooper	...	75 (14)	52 (12)	59 (5)
McGovern	...	75 (14)	51 (7)	56 (10)
Cutoff	...	71 (36)	50 (25)	53 (15)
Cooper to.				
McGovern	...	..	...	...
Cutoff	64 (12)	73 (13)	49 (16)	50 (9)
McGovern to:				
Cutoff	60 (6)	72 (19)	49 (16)	50 (9)

NOTE —Numbers in parentheses are size of group, where N of group $\geqslant 5$
* Entries are dove to nonhawk, dove to hawk, nondove to nonhawk.

new doves were Democrats and that Democrats were disproportionately doves. Senators in both parties generally moved toward dovishness; but less expected is the finding that the Democrats collectively moved toward dovishness in a way very different from the way Republicans shifted. Table 3 shows that of senators remaining in Congress between roll calls, Democrats were generally much more likely to convert to dovishness than Republicans were, and that between the Cooper-Church and McGovern-Hatfield roll calls, Republicans were more likely to shift *away* from dovish positions than Democrats were. However, the last comparisons provide an

interesting example of a "hard-core" phenomenon: there were no Democratic conversions in either of these interludes (in fact, on the Cooper-Church to cutoff comparison, there was substantial movement from dove to nondove), but it is notable that by this time the group of Democratic nondoves consisted entirely of southerners, 12 of whom apparently would not vote dove at all, and six of whom were waverers. With the exception of this group, Democrats proved much more likely than Republicans to convert from nondove to dove, and less likely to convert in the other direction.

Replacement is another matter, however, as the table shows. With the exception of the comparison between the Cooper-Church and the cutoff roll calls, the shift from nondove to dove among replacements was proportionately greater among Republicans.[18] For both Democrats and Republicans, elections led to a substantial decrease in the number of nondoves in office and a corresponding increase in doves. In the aggregate, however, a substantially higher proportion of Republican nondoves was replaced by doves. We know of no work which would have predicted this rather surprising difference between the two parties' patterns of moving from nondove to dove. A plausible speculation might be that Republicans in office—like the southern Democrats referred to above—were more conservative, and consequently less likely to convert, whereas new Republicans were not handicapped by adherence to previously declared positions. It is also possible that Democrats in the Senate were more attuned to changes in public opinion.

Table 4 shows the relationship between voting patterns and age of senators. The table shows that doves were quite consistently younger, on the average, than nondoves, and also that converts—both from nondove to dove and from dove to nondove!—were younger than those who were consistent in their voting patterns. A hypothesis that younger people (although in the Senate "young" is a highly relative term) are more flexible, more likely to change their minds, would thus be supported by the data. This flexibility need not have policy implications, if conversions in both directions cancel out; in this case, however, conversions on balance increase the number of doves.

Of special interest are the ages of the nondoves who left Congress between roll calls; those were consistently over 70 years of age, much older than any other group of senators. This leads to a potentially important question about the way in which the replacement mechanism produced its dovish effect: it may have worked through attrition due to death and

[18] In this preliminary analysis, the unit of analysis is the senator rather than the seat, consequently, the figures presented are aggregate ones. We are comparing the numbers of Democrats and Republicans in office for earlier and later votes, without examining at the seat level how the replacements occurred.

voluntary retirement, rather than through voter rebellion against hawkish incumbents. We will look at this possibility again in table 5.

TABLE 5

FATE OF DOVES AND NONDOVES WHO LEFT THE SENATE BETWEEN PAIRS OF VOTES

Pairs of Roll Calls	1st Vote		% Who Lose* before 2d Vote		% Who Retire or Die before 2d Vote	
	N Doves	N Nondoves	Doves	Nondoves	Doves	Nondoves
Tonkin to:						
Proxmire	2	98	0	7	0	9
Cooper	2	98	100	13	0	16
McGovern	2	98	100	13	0	16
Cutoff	2	98	100	21	0	26
Proxmire to:						
Cooper	10	90	20	7	0	9
McGovern	10	90	20	7	0	9
Cutoff	10	90	20	19	10	21
Cooper to:						
McGovern†	42	58	...	...	..	...
Cutoff	58	42	12	14	9	17
McGovern to:						
Cutoff	39	61	8	16	8	16

* Lose = lose a primary or general election before the second roll call
† Nondoves and doves

Data on seniority are not presented because they are quite similar to (and highly correlated with) those for age, except that it is not very meaningful to discuss the seniority of new arrivals; more senior senators are less likely to be doves and less likely to convert than less senior ones.

Findings on region present no real surprises for those who have studied congressional activity during the Vietnam War. Senators from the northeast were the most likely to be doves, the most likely to convert to dovish positions, and the northeastern states were the most likely to replace nondoves with doves. The north central states were second on all counts, followed by western states (including Alaska and Hawaii), with southern and border state senators least likely to be dovish, least likely to convert, and least likely to be replaced by doves. For example, between the Proxmire and cutoff votes, 81% of available northeastern senators converted from nondove to dove, and 80% of available replacements were doves; at the time of the cutoff vote, only three northeastern senators were nondoves. In contrast, fewer than half of the available southern and border state senators converted to doves or were replaced by doves. In contrast to the analysis of change within the two parties, change processes were the same in all four regions, with conversion and replacement having about the same relative importance in each region.

Elections and Changes in Senate Voting

What was the relationship between the change in Senate voting and elections? Although almost no scholars analyze statistically and systematically the relationship between election outcomes and policy changes, there are many ways one might do so. Table 5 presents one way. Did the public at any point "punish" nondoves or doves by defeating them disproportionately? Although the small numbers require that the data be treated very cautiously, it appears that until near the end of American involvement nondoves were, if anything, *less* likely to be defeated than doves. Although public opinion might have turned against the war, its structure does not appear to have caused the defeat of a disproportionate number of nondoves.[14] Table 5 shows also, however, that although nondoves were not disproportionately defeated, they did in fact leave the Senate more often than doves, because they were more likely to die or retire.[15]

This finding, in conjunction with the data on age in Table 4, leads to new inferences about the relationship between elections and changes in Senate voting. We know the following:

1. Nondoves were disproportionately likely to leave office, but *not* because they were more likely than doves to be defeated in elections; instead, because many of the nondoves were quite old, they were especially likely to die or retire.

2. Particularly toward the end of the war, both doves and nondoves who left office tended to be replaced by doves.

3. Both Democratic and Republican new senators tended to be doves toward the end of the war.

4. The "trend of the times" was toward dovishness, especially toward the end of the war; the effects of both conversion and replacement were to move the Senate from predominantly nondove to dove; and of course the temper of the country was changing as well.

5. Elections were an important part of the Senate change of mind, but not in the way that might be expected. Whatever the realities of particular elections may have been, through most of the war at the aggregate level, the change in Senate dovishness may have been brought about more by the advanced age of many nondoves than by clear electoral choices. Because so few new nondoves entered the Senate toward the end of the war, and

[14] Much theoretical and empirical literature shows that the number of issues involved in most elections is so large, their salience so low, and voter knowledge so poor, that one would expect voters only rarely to defeat legislators because of their stands on a single issue. At times, however, an issue can be so salient and divisive that legislators can be defeated because of their positions (see Miller and Stokes 1966; Page and Brody 1972).

[15] Some might retire "voluntarily" because they expect to be defeated, but it is not necessary to deal with this complication here.

because both Democratic and Republican new senators were doves, we can speculate plausibly that most candidates running were dovish. Thus disproportionate retirements and deaths—not defeats—among nondoves, in conjunction with the dovishness of available candidates, helped produce the increasing dovishness of the Senate. This inference will be tested in future work.

CONCLUSIONS

This article differs from previous work on Congress in three main ways: first, it presents data on the Senate's voting on a single issue, over time; second, it concentrates on the relationship between aggregate-level change mechanisms and *overall* change in Senate composition and policy, enabling us to glimpse important patterns that previous work has overlooked; and third, though clearly preliminary, the analysis and findings are consistent with a number of important hypotheses which together constitute a partial model of political change. We now comment briefly on these three aspects of our approach:

1. We have found that doves were disproportionately Democratic, young, new in the Senate, low in seniority, and from the northeastern and north central parts of the country. We have also noted that the Senate did not pass a significant anti-involvement measure until six years after the Gulf of Tonkin resolution.

2. We have focused, unlike most researchers, on changes in aggregate outcomes rather than on change in individual voting, and so have attempted to study how change in American policy came about, not how changes in individual opinions came about. We have found that replacement of nondove by dove was disproportionately important in the Senate change of mind, especially at the end of the war, but that (under a simple counterfactual assumption, that replacements did not differ in dovishness from incumbents) the differences between incumbents and replacements did not change any of the Senate outcomes we examined, that is, they did not turn potential winning proposals into losers, or vice versa. Nevertheless, in the Senate as in other organizations, replacement proved to be disproportionately important.

3. We have seen that it is possible to draw meaningful conclusions by using such an approach, at least on an issue like Vietnam, which was salient and divisive over much of the period considered here; but now that we know so, a larger investment is both justified and called for. The analysis has led us to hypotheses and interpretations we will examine in future work.

First, the findings are consistent with the assumption that office seekers are rational vote maximizers and the hypothesis that, in order to maximize votes, they will reflect public opinion on salient issues (Downs 1957;

Mayhew 1975). As the war progressed, public opinion turned increasingly against it and the issue became more salient as well (Mueller 1973, chap. 3). If the trends in opinion were uniform in all states and if there were no constraints on the policy stances of senators and challengers, vote maximization would call for both incumbents and challengers to move in a dovish direction. This, in fact, often occurred.

Second, the findings are consistent with hypotheses derived from at least two bodies of literature which would predict that, although both incumbents and challengers should become doves, the rate of change through conversion should be slower than change through replacement. Both Downs's rational officeholder model (1957) and the theory of cognitive dissonance (Shaw and Costanzo 1970, pp. 207–20) imply that those who have already taken public stands on issues will try to be consistent in their future stands. Incumbents who had taken hawkish stands would find it difficult, for political and/or psychological reasons, to take dovish stands. Challengers, however, who presumably were less likely to have taken public prowar stands in the past, would not be under such constraints, and so could adjust more quickly than incumbents to changes in public opinion.

These findings and hypotheses can affect our interpretation of the role played by elections in the political change studied. It is sometimes claimed that popular control of public policy can occur only when opposing candidates differ in their positions on the issues (Sullivan and O'Connor 1972, p. 1257), giving the electorate a meaningful choice at election time. If, however, public opinion is clearly moving in a particular direction on a salient issue, it can be expressed in the election result, and desired policy change can be brought about, even if both candidates take similar positions. One can easily conclude that a specific policy change is the result of a specific election or series of elections; if incumbents and challengers were not concerned with the possibility of defeat, they would have less interest in responding to public opinion. But one need not conclude that elections produce their impact only through the opposition of candidates with contrasting views within particular districts.[16]

[16] The Vietnam issue had a number of characteristics especially likely to lead to the results being discussed Compared to other issues, the impact of the Vietnam confl ct was relatively uniform across states (in contrast to early civil rights legislation, which affected mostly the South, for example, or environmental legislation, which affects producers of power very differently from consumers), the trend of public opinion was relatively clear, and salience was high toward the end of the war Analysis and discussion would probably be more complicated with regard to other issues. An additional consideration not yet mentioned is the possibility that the probable correlation between public opinion and Senate voting is spurious Increasing opposition to the war by both public and Senate might have been due to the fact that casualties, expenditures, and other costs of the war were increasing while few or no tangible benefits were forthcoming. It is also possible that public and Senate opinion responded to the persuasiveness of those who became doves early in the war. Future work will examine these possibilities

Additionally, we have shown that in order to study legislative change, we must consider how elections produce the conjunction of a "demand" side of politics with the "supply" side. To understand change in Senate voting on Vietnam, it is important to see that the correlation of nondovishness with age led to the disproportionate death and retirement of nondoves, creating a *demand* for candidates to fill the seats.[17] Changes in public opinion, etc., created a *supply* of candidates who were mostly doves (while moving incumbents in the same direction), but these candidates produced the effects they did largely because former incumbents created slots for them to fill. Knowing only the demand side or the supply side would not enable us to predict the rate of change in Senate opinion; we must know both.

In this paper we have largely ignored the subjective reactions of the senators involved, attempting instead to understand the process of political change at a higher level of aggregation. The approach is an unusual one for work in the area of congressional voting; the clearest implication of our work to date is that the path indicated by this article deserves to be explored more fully. As might be expected with any new effort, our findings so far have raised questions as well as answered them.

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Inequality and the Relative Size of Minority Populations: A Comparative Analysis[1]

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Socioeconomic differentials separating whites and blacks have been shown to correlate positively with the percentage of blacks in a population. However, in multiracial or multiethnic populations, it is necessary to take into account the effects of the relative size of each minority present in nonnegligible numbers. In the research reported here, the relationship between socioeconomic inequality and the proportion of Mexican Americans and blacks in the population of metropolitan areas was decomposed through path-analytic techniques. Analysis of a model incorporating the impact of the size of both minorities indicates that minority income levels are inversely related to minority size and that disparities between majority and minority income and occupation tend to grow as relative minority size increases. Mexican American occupational levels vary positively with the percentage of blacks, but black occupational status was found to be virtually unrelated to the proportional representation of Mexican Americans in metropolitan areas. Finally, the positive relationship between minority percentage and inequalities of income and occupation persists net of the effects of a number of plausible alternative explanations.

The analysis of socioeconomic inequality, operationalized in terms of differentials between majority and minority populations in levels of occupation and income, has long been central to the study of race and ethnic relations.[2] One of the most consistent findings emerging from the literature on race relations in the United States is that socioeconomic differentials vary directly with the relative numbers of a minority present in a given area—a result predicted in theoretical formulations by Williams (1947) and Allport (1954). The former, for example, concludes that the "dispersion of minorities . . . tends to diffuse hostility, and in the long run, reduce it" (Williams 1947, p. 76). Researchers have found that, virtually without exception, the larger the relative size of a minority, the greater the majority-minority disparities in income (Blalock 1956, 1957; Brown and Fuguitt

[1] The authors are indebted to Harley Browning, Norval Glenn, and Dan Price for careful reading of and helpful comments on earlier drafts. We also wish to thank the anonymous referees whose cogent critiques led to a closer examination of a number of important issues treated herein. As to any errors, nostra culpa.

[2] Of course inequalities in other variables, particularly attainment of formal education, have received much more than passing attention (Blalock 1957; Coleman et al. 1966).

1972). Similar results have been obtained with respect to occupational inequality (Turner 1951; Brown and Fuguitt 1972), although Blalock, in his study of southern counties (1957), observed no relationship between percentages of nonwhites and white-nonwhite occupational differences. As will be discussed in greater detail below, the nature of the relationship involving occupational inequality may be somewhat more complicated than that for income (Glenn 1964, 1965, 1966; Cutright 1965; Brown and Fuguitt 1972).

The major purpose of the research reported here was to extend the study of the structure of the relationship between minority size and socioeconomic inequality to include a consideration of the tripartite relationship among two minorities and the majority[3]—as opposed to the conventional focus on the dominant group and one minority population, usually blacks.[4] This paper reports a comparative analysis of occupational and income disparities in a sample of Mexican Americans, blacks, and Anglos. First, the connection between minority percentage and socioeconomic differentials is investigated separately for blacks and Mexican Americans. This analysis is followed by a simultaneous consideration of variation in the proportionate representation of both minorities in the total population. The analysis focuses primarily on Anglo-black and Anglo–Mexican American *differentials*, but attention is given also to the possible effects of minority size on the socioeconomic *levels* achieved by each of the three groups. In the second part of the analysis, a number of theoretical perspectives are considered which may aid in the interpretation of the findings. To allow some assessment of the tenability of the alternative explanations, the relationship of relative minority size to inequality is examined net of the effects of other determinants of occupational and income disparities.

DATA AND METHOD

The data employed in this analysis are drawn from 1970 U.S. census tabulations on 40 of the 46 Standard Metropolitan Statistical Areas (SMSAs) located in the southwestern United States (U.S. Bureau of the Census 1971, 1972).[5]

Six SMSAs are omitted from the analysis—those situated on the U.S.-

[3] For the first time, to our knowledge.

[4] Of course comparisons of various minority groups along several socioeconomic dimensions have been carried out (e.g., Browning and McLemore 1964; Grebler, Moore, and Guzman 1970; Uhlenberg 1972; Greeley 1974). The point is that the relationship between the relative size of a minority and inequality has not been examined in comparative perspective.

[5] I.e., the units of analysis (SMSAs) are located in the states of California, Texas, New Mexico, Arizona, and Colorado.

Mexico border.[6] To have included them would have introduced a certain ambiguity due to the unusual demographic, social, cultural, and economic character of these areas, shown for example by the multitudinous border crossings by Mexican citizens who enter the United States each day for employment (commuters), business, or pleasure.[7] In three of the six deleted SMSAs (Brownsville, Laredo, and McAllen), Mexican Americans make up an exceptionally large portion of the population (between 76% and 86%). Blalock (1972, pp. 381–83) advises that, where only a few extreme cases are found and when it is impossible to obtain additional extreme cases, "it may be advisable to exclude these from the analysis . . . ." while taking care to report the range of variation in the cases included. (The range of variation is reported below.)[8]

Mexican Americans are identified according to the census designation "Spanish heritage," which, in the five southwestern states, includes the Spanish-language population plus persons with Spanish surnames not reporting Spanish language. Although some fraction of the Spanish-heritage population has Spanish origins other than Mexican, in the Southwest there is a reasonably close correspondence among the results achieved by the application of identifiers such as "Spanish origin," "Spanish surname," or "Mexican origin" (U.S. Bureau of the Census 1973*b*, p. 6). Blacks are identified according to the census code "Negro," and Anglos are defined as nonblack persons of non-Spanish heritage. Data on Anglos were obtained by subtraction of the figures for blacks and persons of Spanish heritage from the totals for each SMSA.[9]

The average proportion of Mexican Americans in the population of southwestern metropolitan areas ($\overline{X} = 14.7\%$, SD $= 10.3$) is somewhat greater than that of blacks ($\overline{X} = 8.1\%$, SD $= 6.8$). The range of values

[6] Brownsville–Harlingen–San Benito, El Paso, Laredo, and McAllen-Pharr-Edinburg in Texas; San Diego, Calif ; and Tucson, Ariz.

[7] For the fiscal year ending June 1970, the number of legal daily border crossings by Mexican citizens totaled 86,699,629, an average of 237,533 per day (U S Immigration and Naturalization Service 1971, p. 37)

[8] As it happens, virtually all conclusions based on a separate investigation carried out for all 46 SMSAs are identical to those derived from the analysis described in the text The only exception is an inconsistent pattern in regard to percentage of Mexican Americans and Anglo income such that the sign of the relationship shifts from positive at low-income levels to negative in the upper-income categories There are also differences in the magnitude of the coefficients, but these support the predictions regarding the minority size/inequality relationship more strongly than does the analysis based on 40 metropolitan areas. However, these more substantial values may result from the "extreme case effect" described by Blalock (1972, pp. 381–83).

[9] Thus oriental Americans are included in the Anglo category. This should not introduce any major distortion, since the group in general (and Japanese Americans in particular) are comparable in most socioeconomic respects to the white population of non-Spanish heritage (Kitano 1969; U.S. Bureau of the Census 1973*a*).

for the minority-size variable is reasonably broad for both: for Mexican Americans, from 0.8% (Texarkana) to 44.6% (San Antonio); for blacks, 0.7% (Anaheim) to 24.1% (Tyler, Tex.). Of the 40 SMSAs, 32 (or 80%) have a population in which the proportionate representation of Mexican Americans ranges from 5% to 25%, while 26 SMSAs (65%) have populations in which the black share of the total ranges from 5% to 25%. Mexican Americans surpass blacks in every income category and at each occupational level except that of professional, technical, and kindred workers, even though the average educational achievement of blacks exceeds that of Mexican Americans. It is obvious that the socioeconomic gap separating Anglos from the two minorities is greater with respect to blacks.

Measurement of the socioeconomic variables follows the procedures outlined by Brown and Fuguitt (1972) in their analysis of 1960 census tabulations, except that data on the Mexican American population are utilized and additional cutting points are employed. Occupational and family-income data for each racial and ethnic group were dichotomized and difference scores calculated by subtracting the proportion of Mexican Americans and blacks, respectively, in each of the occupational and income categories from the corresponding Anglo proportion. Measures of central tendency and dispersion depicting the relative positions of the three groups are presented in table 1.

THE STRUCTURE OF INEQUALITY. DECOMPOSING THE ZERO-ORDER RELATIONSHIP

Recent research by Brown and Fuguitt (1972) has demonstrated that the mechanisms relating minority percentage to indicators of racial disparity are not the same for income as for occupational differentials. Their results are summarized in figure 1. The structure of the relationship for whites is the same for both occupation and income. For the nonwhite component of the model, the structure is not the same for the income and occupation relationships. Percentage of nonwhites is inversely correlated with nonwhite income, which is in turn inversely related to the difference scores. Nonwhite occupational status is negatively related to occupational disparity, but "unlike the situation with income, the greater the percent nonwhite, the greater the proportion of nonwhites in higher occupational categories . . ." (Brown and Fuguitt 1972, p. 580). As a consequence, the zero-order correlation between white-nonwhite occupational differences and percentage of nonwhites is made smaller (but continues positive, nonetheless), since the value is the algebraic sum of positive and negative compound paths.

As Brown and Fuguitt indicate, the observations with respect to income are consistent with what one might expect intuitively, given only the knowl-

TABLE 1

MEANS AND SDs OF VARIABLES INCLUDED IN THE ANALYSIS: 40 SOUTHWESTERN SMSAs, 1970

	% Distributions						Difference Scores			
	Anglo		Mexican American		Black		Anglo–Mexican American		Anglo-Black	
Variables	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD	$\bar{X}$	SD
Occupation:										
Professionals, technicians, kindred personnel	18 4	4 4	8 3	2 5	10 0	3 3	10 1	4 2	8 4	4.6
Foregoing plus managers, proprietors	28.9	4 8	13.1	3 1	12 5	3 8	15 9	4 9	16.4	5.2
Foregoing plus sales personnel	37.9	5 1	17.7	3 5	15 0	4.4	20 2	5 2	22.9	5 9
Foregoing plus clerical personnel	57.1	6 9	31 2	7 0	26.8	9 1	25 9	8 2	30.3	9.8
Foregoing plus craftsmen, foremen	71.2	5 8	45 8	8 1	35 2	10.0	25 4	7 6	35.9	10.5
Family income:										
$8,000+	63.6	9 2	43 6	12 2	32.7	11.5	20.0	10.7	30 9	11.3
$7,000+	70.2	8 9	52.4	12 0	40.6	11 1	17 8	11.7	29.6	11.9
$6,000+	76.2	8 5	61 9	10.5	49.5	11.5	14 3	11.4	26 7	12 9
$5,000+	81 5	8.4	70 4	8 7	59 0	10.5	11.1	10.7	22.5	12.7
$4,000+	86.2	8 4	79 2	5.9	68 6	8.7	7.0	9 7	17.6	12.0

Figure 1a
Occupation

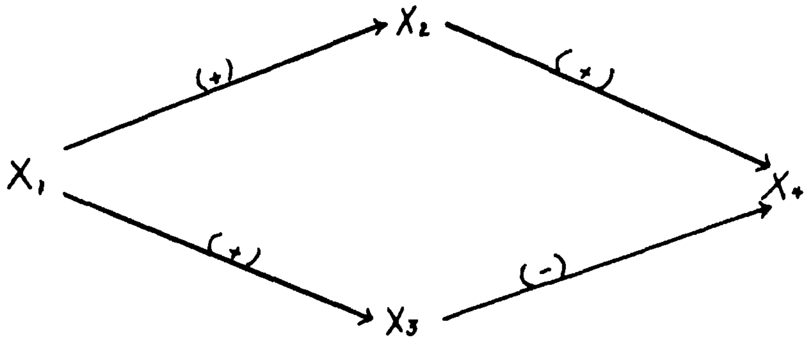


Figure 1b
Income

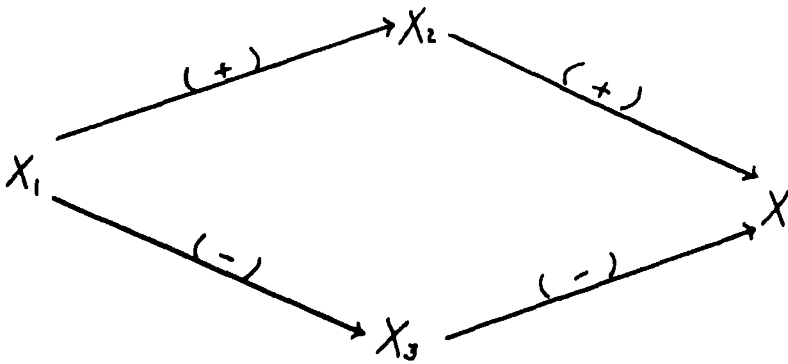


FIG. 1.—Schematic path diagram showing separate decompositions of relationships between minority percentage and occupational and income difference scores, adapted from Brown and Fuguitt 1972. X_1 = proportion of minority in the population; X_2 = majority proportion above a certain occupational or income level; X_3 = minority proportion above a certain occupational or income level; $X_4 = (X_2 - X_3)$.

edge of a positive correlation between the relative size of the nonwhite population and income-difference scores. The results for occupation may help to account for Blalock's (1957) finding that there is no significant relationship between minority concentration and occupational differences. They are also consonant with both the dual (or semiseparate) economy and "overflow" hypotheses which purport to explain the association of percentage of blacks (or nonwhites) with black occupational status (Blalock 1957; Glenn 1964). The former proposition speculates that blacks may benefit occupationally in areas where they constitute a fraction of the total population large enough to stimulate the development of a semiseparate

economy in which blacks patronize professionals and businessmen drawn from their own ranks. Alternatively, the proportionate size of the minority may be great enough to occasion a certain overflow of minority workers from lower- to higher-status occupations. This may occur because virtually all lower-level jobs are already occupied by members of the minority and/or because there is not a sufficient number of majority workers available to fill all upper-status positions (Glenn 1964, pp. 48–49). In addition, there is evidence that, while both the majority and the minority may benefit occupationally from the presence of the minority in relatively large numbers, only the majority—or more precisely, certain subgroups within it—is likely to gain in monetary terms (Glenn 1964; Brown and Fuguitt 1972).

In figure 2, we consider the impact of the relative size of each minority in a single, expanded model. It is apparent that the expanded model permits an investigation of the effects of the proportionate size of each minority on the socioeconomic levels attained by the other and of the combined effects on the status of the Anglo majority. It also allows a partioning of the correlations between minority percentage and difference scores into their respective components. In both parts of figure 2, variables are designated by the same symbol, in order to facilitate the later presentation of results in tabular form. Disturbance terms are omitted from the figures for ease of presentation.

Expected Structural Relationships

It is possible to summarize the underlying expectations regarding structure by a series of specific hypotheses. In the absence of either previous comparative research in this area or any rationale for expecting the structure of Anglo–Mexican American interrelations to differ substantially from Anglo-black relations, the hypotheses focusing on Mexican Americans generally parallel those for blacks. Theoretical perspectives bearing on the expected relationships are discussed in more detail below.

The first of our hypotheses is that there will be a positive correlation linking the relative size of both minorities to Anglo-minority differences. The proportionate size of the Mexican American population is predicted to be inversely related to Mexican American income level, which in turn is inversely related to the magnitude of the divergence in income between Anglos and this minority. (Given the model employed, associations of the latter type are nonproblematic.) The direction of the relationship involving black and Anglo income levels is expected to be the same as that for Mexican Americans and Anglos.

Further, the percentage of Mexican Americans in the population is expected to be positively related to the occupational attainment of that minor-

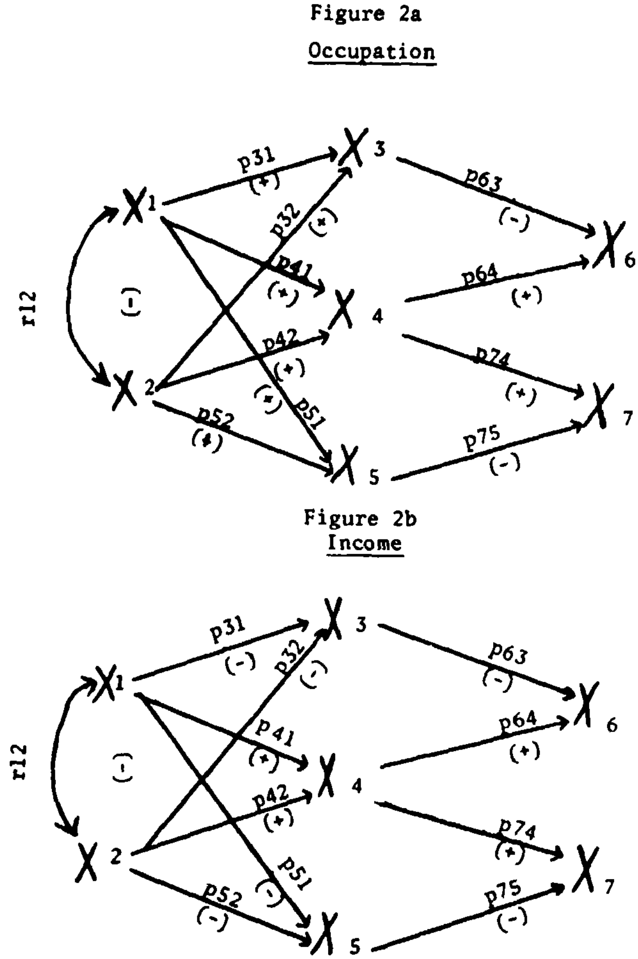


FIG. 2 —Schematic path diagram of expanded model showing decomposition of relationships between relative size of Mexican American and black populations and occupational and income difference scores. X_1 = proportion of Mexican Americans in the population, X_2 = proportion of blacks in the population, X_3 = Mexican American proportion above a certain occupational or income level; X_4 = Anglo proportion above a certain occupational or income level; X_5 = black proportion above a certain occupational or income level; $X_6 = (X_4 - X_3)$; $X_7 = (X_4 - X_5)$.

ity,[10] while the latter variable will vary inversely with Anglo–Mexican American occupational differentials. The same is expected to hold true

[10] A question may arise as to the meaningfulness of a statement proposing opposite effects of minority size on occupational and income levels of the same minority First, one must be clear about what the hypothesis does not say. It does not say that occupational gains will be associated with income losses. Confusion on this point may result if one does not keep in mind that we are working at the macrolevel rather than at the individual level Income may increase along with occupational levels and still be

for blacks. The relative size of each minority is hypothesized to be positively related to Anglo income and occupational levels. Variation in the latter obviously will relate positively to majority-minority differentials.

To this point, all propositions have been derived more or less directly from earlier research, although prior tests have involved only whites and blacks (or nonwhites). We now turn our attention to the effects of the relative size of each minority on the socioeconomic level of the other. The socioeconomic position of Mexican Americans may vary positively or negatively with the percentage of blacks. Blacks may either be advantaged or disadvantaged as the percentage of Mexican Americans rises in the total population of a given area. There may of course be no relationship of consequence, but that alternative appears improbable, given the consistent empirical reaffirmation of the importance of the connection between minority size and socioeconomic status. It is our final hypothesis that a situation in which Mexican Americans experience socioeconomic gains stimulated by increased proportions of blacks in the population is a more likely phenomenon than the reverse. However, this expectation pertains only to occupational achievement. Income benefits seldom accrue, at least partially because of the concentration of the minority in lower-level niches within broad occupational categories.

The rationale for the final hypothesis is that, based on what we know of the position of the two minorities in American society, the probability is greater that Mexican Americans rather than blacks will profit from any occupational opportunities associated with relative minority size. We are explicitly *not* suggesting that a positive connection between Mexican American occupational status and percentage of blacks would represent evidence that Mexican Americans discriminate against or take advantage of blacks. It is simply that the available evidence suggests that Mexican Americans, especially acculturated members of that minority, face fewer obstacles to socioeconomic achievement than do blacks (see Fogel 1965, p. 23; Bogardus 1968, p. 152; Howard 1970).[11] For the reasons cited above, all relationships involving minority size and income are expected to be inverse.

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lower on the average in cities in which the proportion of the minority is greater. The "semiseparate economy" and "overflow" hypotheses, along with the possibility of the concentration of workers in lower-level jobs within broad occupational categories, offer rationales for such a relationship. A major question remains as to how likely it is that at any particular point in time there will exist a comparatively large proportion of a minority in higher-status jobs coupled with relatively few receiving higher-level incomes Although this is not the central issue in the present analysis, it is our intention to test the hypothesis of opposite effects of the minority-size variable suggested by earlier research, especially since a logical case for similar (i e , same-direction) effects can easily (indeed more easily) be made In fact, previewing the results to be presented below, the latter proposition receives considerably more support from our data than does the former

[11] For many purposes, it is idle to attempt to demonstrate that blacks suffer a higher

Analysis of the Two Structural Models

The structural analysis may be partitioned into two interrelated components. The first, designated Model 1, is comparable to the Brown and Fuguitt work in which socioeconomic disparities involving the majority and one minority are examined (as in fig. 1). Model 1*a* focuses on occupational levels and differentials; Model 1*b* deals with income. The simultaneous consideration of both minority-size variables (as in fig. 2) is referred to as Model 2, divided like Model 1 into occupational and income components (Models 2*a* and 2*b*, respectively). The results of an effort to assess the adequacy of the models appear in tables 2–5.[12] As Brown and Fuguitt (1972, p. 575) state, the decomposition treats majority and minority levels of socioeconomic achievement as intervening variables between the percentage of the minority located in an area and the difference scores. The procedures are those basic to path analysis and are described in detail in Brown and Fuguitt (1972) and elsewhere (Duncan 1966; Land 1969; Alwin and Hauser 1975). It should be noted that, although there is a theoretical basis for predicting a nonlinear relationship between minority percentage and degree of discrimination, in general measures of discrimination have been found to be "approximately *linearly* related to minority percentage" (Blalock 1967, p. 156; emphasis in the original).[13]

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degree of discrimination than Mexican Americans. Both groups have been severely disadvantaged through the denial of equal access to social, economic, and legal rights and privileges. Yet in order to understand more adequately the way in which inequality comes about, such a distinction may be highly significant

[12] The manner in which the models relate to figs. 1 and 2 should be clarified. Fig. 1 may be thought of not only as illustrative of Brown and Fuguitt's findings but also as a set of propositions applicable to the discrete investigation of the impact of the relative size of each minority. The information contained in tables 2 and 3 can then be viewed as empirical evidence of the tenability of those propositions in a setting in which two minority populations exist in nonnegligible numbers. Similarly, fig 2 displays the relationships predicted when both minorities are treated in the same model, and tables 4 and 5 present the relationships as they were observed Thus, while figs 1 and 2 summarize the two sets of hypotheses here designated Models 1 and 2, respectively, the findings that actually emerge from a test of the two models must be determined by inspection of the tabular presentations

[13] Failure to detect a significant departure from linearity does not necessarily obviate the conceptualization underlying the anticipation of a curvilinear function Undoubtedly measures of inequality indicate the effects of variables other than degree of discrimination. Further, the observed linear relationship may represent a composite of two forms of nonlinearity associated with the relative size of a minority—one pertaining to economic competition, where discrimination might be expected to increase with a decreasing slope, the other related to a perceived (political) power threat, where discrimination may well increase with an increasing slope (Blalock 1967, pp. 156–58). In any event, socioeconomic inequality is, in itself, a sufficiently important phenomenon to warrant additional analysis, even though measures of inequality tap other underlying variables in addition to discrimination and regardless of whether perceptions of both an economic and a power threat combine to produce an overall linear relationship.

TABLE 2

COMPOUND PATHS BETWEEN % MEXICAN AMERICAN AND DIFFERENCE SCORES THROUGH COMPONENT VARIABLES

VARIABLES	ANGLO COMPONENT			MEXICAN AMERICAN COMPONENT			CORRELATION*
	r_{14} (1)	p_{64} (2)	$r_{14}p_{64}$ (3)	r_{13} (4)	p_{63} (5)	$r_{13}p_{63}$ (6)	$r_{16} = r_{14}p_{64} + r_{13}p_{63}$ (7)
Occupations:							
Professional, technical	0.32101	1.03499	0.33224	−0.18215	−0.59697	0.10874	0.44098
Foregoing plus managers and proprietors	0.38075	0.97061	0.36956	−0.18871	−0.61870	0.11675	0.48632
Foregoing plus sales	0.39249	0.97395	0.38227	−0.08714	−0.67047	0.05842	0.44070
Foregoing plus clerical	0.35017	0.83843	0.29359	−0.07233	−0.86066	0.06225	0.35584
Foregoing plus craftsmen	0.31735	0.76525	0.24285	−0.13756	−1.06398	0.14636	0.38921
Family income:							
$8,000+	0.05988	0.85749	0.05135	−0.16816	−1.13552	0.19095	0.24230
$7,000+	0.03954	0.75902	0.03001	−0.18053	−1.02920	0.18580	0.21581
$6,000+	0.01110	0.74984	0.00832	−0.24364	−0.91892	0.22389	0.23221
$5,000+	−0.01239	0.78015	−0.00967	−0.21443	−0.81908	0.17390	0.16423
$4,000+	−0.02695	0.86559	−0.02333	−0.35245	−0.60976	0.21491	0.19158

* For $r > .28$, $P < .05$.

TABLE 3

COMPOUND PATHS BETWEEN % BLACK AND DIFFERENCE SCORES THROUGH COMPONENT VARIABLES

Variables	Anglo Component			Black Component			Correlation*
	r_{24} (1)	p_{74} (2)	$r_{24}p_{74}$ (3)	r_{25} (4)	p_{75} (5)	$r_{25}p_{75}$ (6)	$r_{17} = r_{24}p_{74} + r_{25}p_{75}$ (7)
Occupation:							
Professional, technical	—0 07009	0 95772	—0 06713	—0.56123	—0 71383	0.40062	0.33349
Foregoing plus managers, proprietors	—0 06647	0 92221	—0 06130	—0 55685	—0 73455	0.40892	0 34762
Foregoing plus sales	—0.05281	0 86279	—0 04556	—0 58136	—0 73598	0 42787	0.38231
Foregoing plus clerical	0 04279	0.69843	0 02989	—0 53391	—0 92811	0 49553	0 52542
Foregoing plus craftsmen	0 20818	0 55038	0.11458	—0.48723	—0 94648	0.46115	0.57573
Family income:							
$8,000+	0 08766	0 81340	0 07130	—0 34220	—1 01849	0 34853	0.41983
$7,000+	0.12032	0 74926	0 09015	—0 36190	—0 93959	0.34004	0 43019
$6,000+	0.16161	0 66139	0 10689	—0.40203	—0 89540	0.35998	0.46687
$5,000+	0 17085	0.66005	0 11277	—0.41730	—0 82582	0 34461	0.45738
$4,000+	0 17351	0 70073	0 12158	—0.41065	—0 72810	0 29899	0.42057

* For $r > 28$, $P < 05$.

TABLE 4

Models 2*a* and 2*b*

Compound Paths Linking % Mexican American and Anglo/Mexican American Difference Scores on Occupation and Income Variables, Taking into Account Proportionate Size of Black Population

Variables	Anglo Component r_{12} (1)	p_{41} (2)	p_{64} (3)	p_{42} (4)	$p_{41}p_{64}$ (5)	$r_{12}p_{42}p_{64}$ (6)	Sum of Anglo Component $p_{41}p_{64}+$ $r_{12}p_{42}p_{64}$ (7)	Mexican American Component p_{31} (8)	p_{63} (9)	p_{32} (10)	$p_{31}p_{63}$ (11)	$r_{12}p_{32}p_{63}$ (12)	Sum of Mexican-American Component $p_{31}p_{63}+$ $r_{12}p_{32}p_{63}$ (13)	Correlation* $r_{16}=$ $p_{41}p_{64}+$ $r_{12}p_{42}p_{64}+$ $p_{31}p_{63}+$ $r_{12}p_{31}p_{63}$ (14)
Model 2*a* (occupation)														
Professional, technical	—.44611	36174	1.03499	.09129	.37440	—.04215	.33225	— 06792	—.59697	.25607	04055	06819	.10874	.44099
Foregoing plus managers and proprietors	—.44611	.43833	.97061	12907	42545	— 05589	.36956	—.06573	—.61870	.27568	.04067	.07609	.11676	.48632
Foregoing plus sales	—.44611	.46060	.97395	.15266	.44860	— 06633	.38227	.01491	— 67047	.22875	— 01000	06842	05842	.44069
Foregoing plus clerical	—.44611	.46101	.83843	.24845	.38652	— 09293	29359	.04800	— 86066	.26974	—.04131	.10357	.06226	35585
Foregoing plus craftsmen	—.44611	51214	.76525	.43665	39192	— 14907	.24285	.05037	—1 06398	.42126	— 05359	.19995	.14636	.38921
Model 2*b* (family income) ·														
$8,000+	— 44611	.12358	.85749	.14279	.10597	— 05462	.05135	—.21522	—1 13552	—.10548	.24439	— 05343	.19096	.24231
$7,000+	— 44611	.11637	.75902	.17224	.08833	— 05832	03001	— 23079	—1.02920	—.11267	.23753	—.05173	.18580	.21581
$6,000+	—.44611	.10387	.74984	.20795	.07789	— 06956	00833	— 26805	—.91892	—.05474	.24632	—.02244	.22388	.23221
$5,000+	—.44611	.07969	.78015	.20640	.06217	—.07183	— 00966	— 28084	— 81098	— 14886	22776	— 05386	.17390	16424
$4,000+	— 44611	.06299	.86559	.20161	.05452	—.07785	—.02333	—.39336	—.60976	—.09171	.23986	—.02495	.21491	.19158

* For $r > 28$, $P < 05$.

TABLE 5

MODELS 2a AND 2b

COMPOUND PATHS LINKING % BLACK AND ANGLO/BLACK DIFFERENCE SCORES ON OCCUPATION AND INCOME VARIABLES, TAKING INTO ACCOUNT PROPORTIONATE SIZE OF MEXICAN AMERICAN POPULATION

VARIABLES	ANGLO COMPONENT r_{12} (1)	p_{42} (2)	p_{74} (3)	p_{41} (4)	$p_{42}p_{74}$ (5)	$r_{12}p_{41}p_{74}$ (6)	SUM OF ANGLO COMPONENT $p_{42}p_{74} +$ $r_{12}p_{41}p_{74}$ (7)	BLACK COMPONENT p_{52} (8)	p_{75} (9)	p_{51} (10)	$p_{52}p_{75}$ (11)	$r_{12}p_{51}p_{75}$ (12)	SUM OF BLACK COMPONENT $p_{52}p_{75} +$ $r_{12}p_{51}p_{75}$ (13)	CORRELATION* $r_{17} =$ $p_{42}p_{74} +$ $r_{12}p_{41}p_{74} +$ $p_{52}p_{75} +$ $r_{12}p_{51}p_{75}$ (14)
Model 2a (occupation):														
Professional, technical	— 44611	.09129	.95772	.36174	.08743	— 15455	— 06712	—.54452	— 71383	.03746	.38869	.01193	.40062	.33350
Foregoing plus managers, proprietors	— 44611	.12907	.92221	.43833	.11903	— 18033	—.06130	—.55470	—.73435	00481	.40734	.00158	40892	34762
Foregoing plus sales	—.44611	.15266	.86279	.46060	.13171	—.17728	—.04557	—.59817	— 73598	— 03769	.44024	—.01237	.42787	.38230
Foregoing plus clerical	—.44611	.24845	.69843	.46101	.17352	—.14364	02988	—.51325	—.92811	04632	.47635	.01918	49553	.52541
Foregoing plus craftsmen	—.44611	.43665	.55038	.51214	.24032	—.12576	11457	— 46260	— 94648	.05522	.43784	.02332	46116	.57573
Model 2b (family income):														
$8,000+	— 44611	.14279	81340	12358	11615	— 04784	07131	— 36628	—1 01849	—.05397	.37305	— 02452	.34853	.41984
$7,000+	—.44611	.17224	.74926	.11637	12905	— 03890	.09015	—.39151	— 93959	— 06637	.36786	— 02782	34004	.43019
$6,000+	—.44611	.20795	.66139	10387	.13754	— 03065	10689	—.42484	— 89540	—.05112	.38040	— 02042	.35998	46687
$5,000+	— 44611	20640	.66005	.07969	.13623	— 02346	11277	—.45944	—.82582	—.09446	.37941	— 03480	.34461	45738
$4,000+	— 44611	20161	.70073	06299	14127	—.01969	.12158	—.47888	—.72810	—.15294	.34867	— 04968	29899	.42057

* For $r > 28$, $P < .05$

EMPIRICAL COMPARISON OF THE TWO MODELS

Space limitations obviously preclude the presentation of results in diagrammatic form. Fortunately, the same purpose can be accomplished in a tabular format in which the values of all paths linking the variables of interest are recorded.[14]

The basic postulate underlying this and similar research (viz., the greater the proportion of a minority in the total population, the larger will be the occupational and income inequalities separating the majority and minority populations) is supported in every instance for both minorities (see the last column in each of tables 2–5) In most cases the zero-order correlations are significant beyond the .05 level The only exception is in regard to Anglo–Mexican American income differentials, and even here all correlations are in the predicted direction.

Models 1*a* and 1*b*: Analysis of the Separate Effects of the Relative Size of the Mexican American and Black Populations

Tables 2 and 3 display the results of the separate decomposition of the association between minority size and socioeconomic inequality for Mexican Americans and blacks, respectively. The data yield support for many of the hypotheses put forward earlier. More notable is the fact that, in certain important respects, a divergence from theoretical expectations was observed. In part, the failure of some of the expectations to be borne out may be attributed to the fact that the data are drawn from a region other than those which have been settings for earlier research (often the South). But the larger part of the explanation of the deviations from the theoretical framework appears to lie in the failure of Model 1 to take into account the combined effects of variation in the size of both minorities. As will be demonstrated later, the findings conform much more closely to predictions when the size variable for both minorities is entered into the system of equations.

As predicted, the percentage of Mexican Americans is positively correlated with Anglo occupational level (see table 2). The same holds true for Anglo income, with the exception of the two lowest categories. In terms

[14] Throughout this discussion, it is important to remember the caution advanced by Brown and Fuguitt regarding models of the type utilized here I e, little if any significance should be attached to a comparison of the relative magnitudes of the Mexican American, black, and Anglo compound paths, because certain of the path coefficients "are an element in their computation, and the relative size of the latter measures appears to be influenced by the location of the cutting point" (1972, p 579). This caveat does not apply to all coefficients, nor is it an especially serious problem overall, since the directions of the respective compound paths are remarkably consistent within each model In particular, the problem does not arise with respect to the direct effects of the relative size of each minority.

of the model, Anglo socioeconomic levels must be positively related and Mexican American levels negatively related to the difference scores (and similarly for blacks). The relative number of Mexican Americans residing in southwestern metropolitan areas is inversely correlated with Mexican American income, but contrary to what was anticipated, the former variable correlates inversely with the occupational status of this minority.

The hypotheses focusing on percentage of blacks and Anglo-black income disparities are fully supported (table 3). The proportion of blacks in the population is positively correlated with Anglo income and negatively correlated with black income. However, percentage of blacks is inversely correlated with black occupational status. Even more surprising is the inverse correlation between black proportions and Anglo occupational status for the three highest categories (although the relationships are for the most part quite weak). Rather than attempt to interpret the anomalous findings at this point, it will be expedient to consider the results of the expanded model.

Models *2a* and *2b*: Analysis of the Combined Effects of Relative Size of Mexican American and Black Populations

Several important observations serve to illustrate the analytical advantage offered by the development of the expanded model (tables 4 and 5). First, consider the moderately strong correlation between percentage of blacks and percentage of Mexican Americans ($r = -.446$, $P < .01$). In areas where Mexican Americans constitute a comparatively large portion of the total population, black representation tends to be small, and vice versa. Apparently this pattern has existed for a long period of time and may be explained by the historical entrenchment of one of the minorities in the economies of southwestern metropolitan areas, which leaves few opportunities to attract members of the other group (Grebler et al. 1970, p. 117).[15]

One of the more striking results in tables 4 and 5 is that most of the hypothesized relationships which failed to materialize in Model 1 are supported when both minority-size variables are incorporated into Model 2. In particular, it is observed that (1) the relationship between the relative size of the Mexican American population and Anglo income (r_{14} in table 2, p_{41} in table 4) is now positive at all levels, as predicted (table 4, col. 2); (2) the anomalous finding in Model *1a* of an inverse correlation between black proportions and Anglo occupation in the three highest cutting points (r_{24}, table 3, col. 1) becomes positive in Model *2a*, as hypothesized (p_{42}, table 5, col. 2); (3) the negative correlation linking the percentage of

15 Grebler et al. (1970) make this point especially with respect to occupancy of low-paying jobs.

Mexican Americans to the occupational status of that minority (r_{13}, table 2, col. 4), which was contrary to the expected direction, is reversed in accordance with the hypothesis in Model *2a* (p_{31}, table 4, col. 8), except for the two highest occupational levels. However, the coefficient is in every instance so small that the most reasonable conclusion is that there is little or no effect. Thus nearly all the hypotheses set out originally are sustained in the analysis based on Model 2.

One other issue remains to be addressed. In Model *2a*, as in Model *1a*, the proportion of blacks in the population continues to be inversely associated with black occupational levels rather than positively related, as anticipated. Comparisons across cutting points are not especially relevant, because for all occupational categories the relationship is consistent in strength as well as in direction (see r_{25}, table 3, col. 4; and p_{52}, table 5, col. 8). This result represents the only remaining significant departure from earlier empirical findings. Although no single, completely satisfactory interpretation of this irregularity is available, plausible explanations may be offered. Blacks may simply be more occupationally disadvantaged by Anglo discrimination in the Southwest than in most other regions. A more theoretically meaningful interpretation is suggested by the fact that Mexican Americans apparently benefit occupationally in areas in which blacks make up a large part of the population. Across all levels, the relationship between percentage of blacks and Mexican American occupational status is consistently strong and positive (p_{32}, table 4, col. 10), while the path coefficients connecting black occupation to percentage of Mexican Americans (p_{51}, table 5, col. 10) are so uniformly small as to suggest the absence of any relationship. It may be that Mexican Americans are more firmly entrenched in the economic system of the Southwest, since their historical roots in the region antedate those of blacks (and of Anglos, for that matter).

By far the most straightforward interpretation, however, is that the observed effects of the relative size of a minority on the occupational level of the same minority (negative in the case of blacks and virtually nonexistent in the case of Mexican Americans) do not represent "irregularities" at all. Both intuitively and theoretically, it is appealing to conclude that the direction of the effect is the same for both occupation and income, inasmuch as the latter two variables are positively related and, other things equal, rising occupational levels can be expected to increase income. The noneffect of the size of the Mexican American population on Mexican American occupation may be explicable in terms of an "overflow" effect partially offsetting what would otherwise be a moderate to large negative impact of a larger Chicano population. Such a conclusion gains credence from the evidence that structural conditions (Fogel 1965) and majority attitudinal dispositions (Bogardus 1968; Howard 1970) are less favorable to the

achievement of blacks than of any other minority, including Mexican Americans. The finding that the sign of the path connecting percentage of Mexican Americans and Mexican American occupational levels is negative only for the two highest categories is likewise consonant with this reasoning since it is also plausible that the majority will attempt to restrict the overflow to lower-status jobs. If the process does indeed operate in this fashion, it is but a short step to the inference that it is mainly middle- to upper-class whites who gain from the subordination of minorities (see Reich 1971).

It appears that Mexican Americans are not able to convert occupational gains associated with increases in black proportions to monetary advantage (p_{32}, table 4, col. 10). Similarly, black income gains are not forthcoming from increases in the proportionate size of the black population (p_{52}, table 5, col. 8) or from augmentation of the numbers of Mexican Americans (p_{51}, table 5, col. 10).

With respect to the other elements of each specific compound path, little remains to be said, because the structure of the relationships is quite similar to that previously described in Model 1. The zero-order correlations between minority percentage and socioeconomic disparities (tables 2 and 3, col. 7; tables 4 and 5, col. 14) are of course exactly the same in both models.

THEORETICAL PERSPECTIVE ALTERNATIVE EXPLANATIONS OF THE RELATIONSHIP BETWEEN RELATIVE MINORITY SIZE AND INEQUALITY

Since explanations of the positive association between minority percentage and inequality have so often been founded on the assumption of majority discrimination, we shall first explore interpretations based on this proposition. Second, we will examine other plausible explanations.

1. Explanations Based on Discrimination

The reasons for the oft-noted relationship linking minority percentage to measures of inequality may have to do simply with the availability of the subordinate group. Given that discrimination exists, then ceteris paribus one would expect that the more members of a minority relative to the total population available as, say, a cheap source of labor, the greater the benefits that will accrue to the majority (Marshall 1974, pp. 866–67).

Another explanation (Blalock 1967) posits that the larger the relative size of a racially or culturally distinct minority, the more likely will be the majority to perceive an economic and/or political threat emanating from that minority. It might be expected, then, that discriminatory behavior by the majority will be more intense where minority percentages are high. The hypothesized outcome is that, "provided the discriminatory behavior actually results in handicaps for the minority, the larger the minority per-

centage the greater the average gap between the two groups . . ." (Blalock 1967, p. 148). However, evidence adduced by several scholars suggests that only certain sectors of the white population, principally employers and higher-level employees, gain from minority subordination, while working-class whites may suffer negative economic consequences (Glenn 1966; Reich 1971; Dowdall 1974).

The difficulty experienced by blacks in achieving financial advantage is perhaps accounted for by the fact that members of this minority do not always receive compensation commensurate with their occupational status or performance (Billingsley 1968, pp. 88–90). There is also a tendency for blacks to be relegated to less remunerative positions toward the bottom of broad occupational categories (Marshall 1974, p. 867; Brown and Fuguitt 1972, p. 581). Mexican Americans too tend to be disproportionately concentrated in poorer jobs within occupational groups. Moreover, occupational earnings ratios adjusted for within-category concentration indicate that, besides holding jobs that are characteristically low paying, Chicanos are disadvantaged through lower wages for roughly comparable work (Grebler et al. 1970, p. 235).

Different interpretations of the mechanisms and consequences of the observed inequalities may be identified. For example, it has been suggested that, "in addition to the technological and market forces producing labor market segmentation" and relegation of large numbers of minority workers to the secondary labor force (see Piore 1972), capitalist interests rationally and deliberately stratify the market for labor, thereby creating divisiveness among workers and preventing the emergence of class consciousness which, in turn, limits or forestalls entirely concessions that employers might otherwise have to make to a more united labor front (Gordon 1972).[16] It is not necessary, however, to assume a deliberate attempt by employers to create cleavages along racial or ethnic lines but only to assume that "racism is symbiotic with capitalistic economic institutions." Irrespective of whether employers attempt to undermine worker solidarity, the predicted outcome is the same: "Capitalists gain while workers lose." In contrast, the "neoclassical" perspective suggests that, although (or because) racism places white workers in an advantaged position, inefficient use is made of labor, with the result that "capitalists lose and white workers gain . . ." (Reich 1971, p. 110).

All the explanations presented thus far rely to some extent on the assumption that discrimination accounts for the major share of majority-minority socioeconomic differentials. All are consistent with findings that minority percentage varies directly with the degree of inequality. But clearly inequality is not synonymous with discrimination. There are an indefinite number

[16] Marshall (1974) provides an excellent review and critique of economic theories related to racial inequality.

of competing explanations (Blalock 1972, pp. 112–14), not all of which can be specified, much less examined. However, it will be useful to consider a number of alternative hypotheses, because the relationship between minority percentage and inequality may weaken or even disappear when other relevant variables are controlled.

2. Alternative Explanations

Recent research indicates that black migrants are "economically superior" to nonmigrants (Villamez 1976; Long and Heltman 1975; Weiss and Williamson 1972), and it is plausible to assume a similar selectivity to be operating with respect to Mexican Americans. On the one hand, since it is reasonable to expect a tendency on the part of the minority to move to places where economic opportunities are relatively great, ceteris paribus the gap in occupation and income might be expected to diminish or, at worst, remain constant as minority size increases. On the other hand, if both majority and minority populations have tended to redistribute in the direction of areas characterized by economic growth and if the benefits of that growth are disproportionately reflected in majority socioeconomic status, the historic accumulation of the minority would vary positively with inequality, even though the position of the minority at destination represents an improvement over that at origin. Such a situation need not result from discrimination prompted either by capitalist malevolence or by majority perception of an economic or power threat related to minority size. Instead, the observed economic disparity might be due to overall differences between majority and minority in education and skill, a lack of fit between available opportunities and qualifications of minority workers, and/or a tendency for a smaller, more select minority subgroup to locate in areas to which the primary migration flows are *not* directed.

A somewhat related possibility is that places characterized by a low level of discrimination may attract migrants from areas where discrimination is more intense. In this case, disparities in income or occupation at destination would obviously not be a function of minority size leading to suppression at destination but might be a function of such factors as those mentioned immediately above. The latter may or may not be affected by discrimination at origin, but whatever the case, it would be illogical to conclude that relative minority size acts as a determinant of the degree of inequality. While it is beyond the scope of this paper to confront all the issues associated with this line of reasoning, it is possible at least to control for average level of minority educational achievement.

Two additional alternative explanations may be offered. First, Turner, in showing that the industrial character of a city affects the magnitude of inequality, concludes that "manufacturing cities are marked by relative

intergroup equality of occupations," while cities in which service industries are more prominent "are marked by gross (intergroup) inequality of occupations" (1951, p. 528). In this more egalitarian setting, income differences might also be expected to decline. Finally, the rate of increase of a minority population might be positively related to inequality, since rapid growth of the minority may eventuate in a greater volume of workers than either the number of occupational niches or the social infrastructure can accommodate However, Blalock (1956) reports a negative correlation between minority increase and inequality. Regardless of the direction of the effect, it will be instructive to add a measure of minority growth (or migration) to the equation.[17]

The Relationship Net of the Effect of the Alternative Explanations

For each SMSA, the proportion of the labor force engaged in manufacturing, the proportion employed in services, and the median level of education achieved by each minority were incorporated into the analysis, as was the rate of increase between 1960 and 1970 of the black and Mexican American (Spanish-heritage) populations, respectively. The result was that the relationship between relative minority size and inequality remains unchanged in every instance. Table 6 displays the findings in terms of partial correlation coefficients. The variation in the magnitude of the coefficients from one category of occupation or income to another is partly a function of the choice of cutting points, thereby precluding a comparison of, say, the coefficients of the sales and clerical categories or of the $7,000 and $8,000 income groupings. However, a comparison of each particular adjusted value with the corresponding zero-order value is unambiguous. The general tendency is for the size of the relationship between minority percentage and socioeconomic differentials to be reduced by the controls (though this does not always occur). But with only a few exceptions, the relationships which were significant in the zero-order continue to be significant at the same level of confidence in the partials. Moreover, when standardized partial regression coefficients (betas) were computed, the direction of all relationships remained the same, requiring no alteration of conclusions.

Although space limitations permit neither presentation nor extended discussion of the specific effects of each of the control variables, it is interesting to note that most correspond rather closely to earlier findings. The percentage of the labor force engaged in manufacturing is negatively related, and the percentage employed in services tends to be positively related to both income and occupational inequalities. Educational levels are, as

[17] For the total population in the 1960–70 interval, overall rate of population change correlates so highly with percentage of change due to net migration ($r = 97$) that the two variables seem virtually interchangeable.

TABLE 6

ZERO-ORDER AND ADJUSTED VALUES FOR RELATIONSHIP BETWEEN MINORITY % AND ANGLO-MINORITY OCCUPATIONAL AND INCOME DIFFERENTIALS

VARIABLE	MEXICAN AMERICANS		BLACKS	
	Zero-Order Value	Adjusted Value*	Zero-Order Value	Adjusted Value†
Occupation:				
Professional, technical	.4410	.3864	3335	.4883
Foregoing plus managers, proprietors	4863	.4131	.3476	4966
Foregoing plus sales	.4407	.3385	.3823	.5252
Foregoing plus clerical	3558	.2199	.5254	.4942
Foregoing plus craftsmen	3892	.1791	.5757	.4669
Family income:				
$8,000+	.2423	.2965	.4198	.4001
$7,000+	.2158	.2207	.4302	.4244
$6,000+	2322	2738	.4669	.4273
$5,000+	.1642	2455	.4574	.3893
$4,000+	1916	.4815	4206	3149

* Partial correlations with the following controlled for % labor force employed in manufacturing, % labor force employed in services, 1960–70 % change in Mexican American population, Mexican American median education, % population that is black. Data on % change in the Mexican American population missing for 14 cases computations based on pairwise deletions, for $r \geq 35$, $P < 05$ When computations are based on listwise deletions, no changes in direction of relationships result In fact, in the vast majority of cases, adjusted relationships were stronger than those reported in this table

† Partial correlations with the following controlled for % labor force employed in manufacturing, % labor force employed in services, 1960–70 % change in black population, black median education, % population that is Mexican American Data on % change in the black population missing for one case, computations based on pairwise deletions, for $r \geq 28$, $P < 05$ (concerning listwise deletions, see preceding note).

expected, inversely related to socioeconomic differences. The association between the change in the size of the minority populations from 1960 to 1970 and inequality tends, with few exceptions, to be inverse. For present purposes, however, the most important point is that, whether one focuses on partial regression coefficients or partial correlations, the positive relationship between minority percentage and both occupational and income inequality persists in the case of both minorities.

CONCLUSIONS

The structure of inequality among Mexican Americans, blacks, and Anglos has been examined through the decomposition of the effects of the relative size of each minority on majority-minority occupational and income differentials. In the most basic sense, neither minority is "favored": significant socioeconomic inequalities distinguish both from Anglos, and those inequalities are significantly correlated with minority percentages. From a model incorporating the size effect of both minorities, it was concluded that, contrary to what has been suggested by previous research, neither Mexican Americans nor blacks seem to realize occupational benefits from increases in the proportionate size of their own group. Also important is the positive association between Mexican American occupational status and the pro-

portion of the population that is black. Consistent with expectations was the observation that the connection between minority size and minority income levels is inverse. In terms of the objectives of the research, the most important conclusion is that the relative size of minority population emerges as a robust predictor of inequality.

The discrimination hypothesis is strengthened (although by no means proven) by the persistence of the positive association between minority percentage and inequality even in the face of controls for a number of alternative explanations, none of which is necessarily based upon the assumption of discriminatory behavior by the majority.

It would be presumptuous to claim that the results obtained can be generalized without qualification to systems of interrelationships among groups not included in this research. For example, a minority-size threshold may exist (and may vary from one minority to another, depending on such factors as visibility and the demand for the labor of the minority) below which the phenomena analyzed herein are not manifest (Blalock 1957, p. 678). Nevertheless, a major implication of this research is that, in areas in which more than one minority is found in nonnegligible numbers, the impact of the relative size of each minority on the life chances of the other (or others) and on the degree of majority-minority occupational and income inequality must be taken into account.

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Commentary and Debate

SOCIAL STRUCTURE, HOMOGENIZATION, AND "THE PROCESS OF STATUS ATTAINMENT IN THE UNITED STATES AND GREAT BRITAIN"[1]

This comment rests on the premise "that our discipline [sociology] will be judged in the last analysis on the basis of the substantive enlightenment which it is able to supply about the social structures in which we are enmeshed and which largely condition the course of our lives" (Coser 1975, p. 698). For this reason my sympathies lie more with Boudon's defense (1976) of theory as the understanding of social processes than with Hauser's (1976) apparent concern with technique and precision. The comment also proposes two theses. First, bereft of a theoretical framework, the results of data analysis are more frequently a product of technique (statistical, measurement, and sampling) than of underlying social reality. Mathematical or statistical description frequently imposes a sociologically arbitrary model upon social phenomena instead of reflecting them. Wallerstein (1974, p. 8) refers to such methodological gymnastics as "the inversion of the scientific process." The second thesis is that a theory of social structure should be concerned with the emergence, transformation, and interconnection of the pattern of "empty places" between which individuals move. Moreover such a theory is necessarily prior (both logically and empirically) to a theory of social mobility, by which I mean the processes of distributing individuals into particular empty places.

In the light of the above I propose to examine a paper by Treiman and Terrell, "The Process of Status Attainment in the United States and Great Britain" (*AJS*, November 1975). I have selected this article because its presentation is so lucid. The authors are honest in explaining and justifying their statistical manipulations and in conveying information about the data they use. In that it allows and encourages debate, the paper is a model of scientific analysis. Indeed it is its strengths that throw its weaknesses into relief. For it becomes clear, as I argue in the first part below, that Treiman and Terrell sacrifice understanding on the altar of technique. By conceptual and methodological fiat they impose homogeneity upon heterogeneous social structures. In the second part I indicate why it is impossible for them to draw any meaningful conclusions about status attainment without referring to the structural context of mobility.

[1] I should like to thank Terence Halliday, Edward Laumann, Kathleen Schwartzman, Arthur Stinchcombe, and Ida Susser for their comments and encouragement. Of course, all the responsibility rests with me.

Finally, in the third part I use one of their more questionable conclusions to demonstrate the fallacies of their approach.

Three Processes of Homogenization

1. *The nature of the sample.*—In the first paragraph Treiman and Terrell state their interest in status attainment within the total society: "The belief that societal values influence social stratification systems is so intuitively comfortable that it has hardly been tested. A case in point is the comparative work on the stratification systems of the United States and Great Britain. The two countries are typically characterized as highly similar in structure but substantially different in value orientations. . . . These theoretical discussions, however, have failed to specify at the empirical level how the pattern and process of social mobility might be expected to vary in the two societies, if at all" (pp. 563–64). Yet their target population contains only "white male heads of households aged 25–64 in the civilian labor force" (p. 565). How do Treiman and Terrell justify confining themselves to such a restricted population?

> The restrictions are based on the following considerations: (1) For the United States, data are available only for males and, in any event, the process of status attainment cannot be assumed to be similar for men and women. (2) Age is restricted to avoid problems created by the facts that many younger men are still in school or in the military and most older men are retired (3) In the British sample, income data are available only for heads of households. (4) Racial differences in the process of status attainment are known to be so substantial in the United States (Duncan 1968; Treiman and Terrell 1975) that we thought it best to restrict our comparison to the majority populations of the two countries. [Pp 565–66]

(Of course, the population from which Treiman and Terrell draw their samples is in fact considerably less than half the total population aged 25–64.)

Treiman and Terrell adopt three types of justification: practical, methodological, and substantive. I deal with the methodological aspects in section 2 which follows. About the practical problems of adapting the results of a survey to purposes for which it wasn't designed, there is little to say, except that it may *not* be better than no survey at all. The substantive justifications are more interesting and revealing. Because the process of status attainment is different for men and women (point 1) and for whites and blacks (point 4), the authors argue for the exclusion of blacks and women from the target population. This is a truly remarkable position. What is so special about white males that makes them more significant than blacks and women? Is it that Treiman and Terrell think the latter don't really belong to American and British society? Why stop at blacks

and women? Why not discard the poor, whose process of status maintenance is likely to be very different from that of the rich (for whom the inheritance of wealth is important)?

The point of analyzing a society is not to eliminate its internal variation but to understand it. To the extent that blacks and women do indeed exhibit different patterns of status attainment, it becomes imperative to include rather than exclude them. That blacks constitute a quarter of Treiman and Terrell's residual population in the United States, whereas the figure for the United Kingdom is more like 5%, cannot be silently ignored when the goal is cross-national comparison. If one discards what is peculiar and focuses on what is common to different societies, it can come as no surprise that they turn out much the same. One would be unlikely to uncover much variation between the processes of status attainment in the United States and those in an underdeveloped country with a predominantly rural population if one confined the analysis to the civilian labor force aged 25–64. For in an underdeveloped nation such a subpopulation, representing the advanced sector of the economy, is the one which most closely resembles industrial nations. As Max Gluckman once said, "A miner is a miner." Those sections of the population which Treiman and Terrell ignore are the very sections most likely to lend a given society its distinctiveness.

The basis for restricting the age of the target population (point 2) reveals another conventional bias, namely, that status attainment refers only to "productive" or "gainful" employment. Students, retirees, housewives, househusbands, and those in the military do not, it appears, have a status as far as Treiman and Terrell are concerned. Instead the authors adopt the dominant ideological view that the activities of such men and women do not constitute occupations. But ideology should not get the better of sociological sense. Such persons occupy significant places in the social structure, and their arbitrary removal from the analysis of status attainment can only distort our understanding of stratification systems.

2. *The use of linear statistics.*—But there is another excuse for the arbitrary exclusion of some groups and a mechanical mastication of the heterogeneous (at least by class) remainder into a single-path model, namely, inadequate methodology. If regression analysis cannot handle discontinuous or nonlinear functions, then it is necessary to rethink methodology and adopt more (or less?) sophisticated techniques rather than make untenable, ad hoc simplifications. Otherwise there is a danger of a situation in which the "methodological tail wags the substantive dog" (Coser 1975, p. 692).

If the social world does not conform to the assumptions of linear regression (as I suggest in the final part), then the world that method creates—and it always creates one—certainly does. That is at once the

appeal and the danger of linear statistics. Other techniques of data analysis, such as Goodman's development of hierarchical models, relax the assumptions made about the world. Whereas these techniques (Goodman 1970, 1972*a*, 1972*b*, 1973, 1974) are mathematically elegant and statistically superior, they are limited to qualitative data, can encounter problems of interpretation, and rest on notions of statistical independence. Thus, we must also develop models which directly mirror our understanding (however crude it may be) of social processes. The mappings we adopt to translate social data into algebraic systems in and of themselves constrain the possible inferences which can be drawn: inappropriate mappings can only lead to fallacious conclusions For all its inelegant mathematics and quantitative imprecision, Boudon's work (1973*a*, 1973*b*) exhibits a sociological sensitivity in the choice of mappings. It is a bold move in the right direction.

Whatever it is, social reality is certainly not an artifact of statistical technique. Either by eliminating nonlinearities and discontinuities or by turning them into linearities and continuities by methodological fiat, Treiman and Terrell have successfully effected an artificial convergence between status attainment in Britain and in the United States (though of course their methods do not obscure all discrepancies). Is sociology to illuminate statistics or statistics to illuminate sociology?

3. *The use of standard scales.*—Treiman and Terrell take to heart Duncan's admonition that "comparative research must employ rigidly standardized procedures" (Duncan 1966, p. 83). But the costs are considerable. The act of standardization also eliminates significant variation. Let us take as an example the Standard International Occupational Prestige Scale. Whereas it is true that both within and between societies people tend to rank occupations similarly, it is not true that in every society occupation is as important a measure of status as it is in the United States. In Japan, for example, whom you work for is more important than what you do. "If you meet a Bradford English Electric foundryman on a train and ask him what he does, the first thing he will probably say is that he is a foundryman, the second thing he comes from Bradford, and the third that he works for English Electric. His Japanese counterpart would most naturally define himself first as a member of Hitachi Company, secondly as working at such and such a factory, and thirdly as being a foundryman" (Dore 1973, p. 115; see also Cummings and Naoi [1975] for a statistical treatment of status attainment in Japan). In other words, as regards status, variation within occupations can be more significant than variation between occupations. Similarly, in Britain the class to which one belongs is probably a more significant measure of status than one's occupation. In tying status attainment to their ethno-

centric scale Treiman and Terrell miss such central characteristics of particular stratification systems. The same obviously applies to their "effects proportional" measure of educational attainment. In Japan education is as important in determining your employer as it is in allocating you to an occupation.

Such subtleties, which lend a particular system of stratification its specific character and structure, are obliterated in the process of standardization. We observe yet again how Treiman and Terrell, rather than allowing similarities and differences to emerge from careful study of particular societies, impose uniformity upon them all. In the blind pursuit of standardization, they appear to have stumbled into the pit of homogeneity.

Three Reasons for Incorporating Social Structure

In the following three sections I argue that the interpretation of status attainment can be undertaken only with reference to the historically specific social structure in which it occurs—in particular the patterns of empty places which define the educational and occupational structures.

1 *Cohort analysis and changes in the social structure*.—To the extent that observed patterns of mobility can be accounted for in terms of changes in the occupational structure, many of the conclusions drawn by Treiman and Terrell—for example, that education fosters social mobility (p. 577)—become suspect or at least uninteresting While it is true that the overall increases in occupational status between father and son are of the order of 5%–10% (table 3, p. 573), such an average conceals the considerable movement around the average. Transformation of the occupational structure can take place without necessarily giving rise to significant changes in average occupational status. Furthermore, because the authors deal with generational rather than cohort data, the averages don't mean a great deal (Duncan 1966, pp. 54–63).

As soon as the effects of changes in occupational structure become a focus of interest, one must control for father's age and son's age, that is, introduce a cohort analysis. To combine within the same sample fathers born 120 years ago alongside those born 55 years ago without distinguishing between them can only lead to confusion. Occupations change their relative status over a period of 65 years. It is equally fallacious to treat education received in the 1920s on an equal footing with that received in the 1950s. A university degree takes on a meaning according to the time it was received. Indeed, the British system of education was transformed in 1944. We hear little about the problems of lumping together those who received their schooling before and after the inauguration of

the "11+" examination. (In n. 13, p. 576, Treiman and Terrell note the problem but claim they can't do anything about it because of the age and size of the sample.)

Treiman and Terrell, therefore, imply that the relationships of their model are invariant or change so slightly that they may be ignored. Even in the United States this is by no means the case. In a study of black and white male cohorts Richard Freeman (1975) has recently shown that it is only older blacks who conform to the conventional wisdom that blacks experience difficulty in the intergenerational transmission of occupational status (see also Featherman and Hauser 1976). For an older cohort of black males there is indeed a weak relationship between father's occupational status and son's educational attainment, but for the younger cohort the relationship is much the same as for their white counterparts.

Just as standardization imposes uniformity on cross-national comparisons, so it also seems to have put a stop to history. What are of interest are not global (and therefore largely meaningless) averages but variations around the average which reflect differences between societies and over time. This requires an analysis of cohorts and social structures. Faced with the noncoincidence of generations and cohorts, Duncan (1966) argued for status-attainment models in which intergenerational mobility is replaced by intragenerational mobility. In this way he hoped to avoid problems of changes in the social structure, particularly the occupational structure. What I am now suggesting is that even in the analysis of status attainment it is necessary to incorporate changes in the occupational structure. I have already pointed to some reasons why this cannot be avoided; I will now propose others.

2. *What is a sociologically sensible baseline model?*—What meaning can one attach to the value of a correlation coefficient, a regression coefficient, explained variance, and so on? On what grounds can one say that a correlation coefficient of .3 is small? What is the baseline value against which we compare it? How does one understand the statement that a unit change in father's occupation is expected to give rise to a change of 0.028 SD in son's education? How does one interpret an R^2 of .086? Either the 8.6% explained variance is compared with the sociologically absurd model according to which father's occupation explains the entire variance of son's education, or no standard is invoked at all and the reader is persuaded that the figure is in some sense small. But small compared to what? What is the baseline model? What should it be?

All I am suggesting is that the values of regression coefficients are themselves sociologically meaningless. ("For so it is Oh Lord, My God, I measure it but what it is I measure I do not know" [Saint Augustine, cited in Coser 1975, p. 692].) Their interpretation can be undertaken only in relation to the values they would be expected to assume in some

sociologically sensible model. One such model might be that which, by some mathematical criterion, minimized mobility consistent with changes in the occupational structure. Another possibility would be a model that maximized such movement. Without some baseline model, incorporating sociological postulates, the actual values of statistically constructed parameters are impossible to interpret. Thus Goodman's models of social mobility (Goodman 1965, 1968, 1969*a*, 1969*b*), in contrast to most linear regression models, invariably analyze the statistical significance of discrepancies between observed values and maximum-likelihood estimates of a model which makes sociological sense. In claiming that education is an instrument of social mobility, Treiman and Terrell are making a sociologically vacuous assertion since they make no reference to any baseline model which would render their results meaningful. Nor do they have any overall measure of the statistical fit of their model (see also Goodman 1976). Moreover, any reasonable baseline model would take into account changes in the occupational structure, changes which they have chosen to ignore.

3. *Toward a theory of social structure?*—The insights of White (1970), Bartholomew (1973), and Boudon (1973*b*), among others, that the movement of individuals cannot be analyzed independently of the structure of places through which they move, appear to be lost on Treiman and Terrell. Duncan (1966, pp. 70–76), however, shows that according to one measure 83% of intergenerational mobility can be attributed to changes in the occupational structure. Recently Hauser et al. (1975*a*, 1975*b*) compared two models in which the relative mobility chances of American men did and did not vary over time. They showed that the second was statistically the more adequate and concluded that the changing occupational structure was responsible for changing mobility patterns. They exhorted students of mobility to direct their attention to the sources and consequences of the transformation of the occupational structure. In other words, a theory of changes in the occupational structure is prior to a theory of the distribution of individuals into places which constitute that structure.

The implications are radical. We can no longer rest content to merely describe status attainment with the aid of linear statistics. Instead we must construct dynamic models which mirror and explain structural change. In Boudon's words, "We must go beyond the statistical relationships to explore the generative mechanisms responsible for them. This direction has a name: theory. And a goal: understanding" (1976, p. 1187). We must embark on nothing less than a theory of the dynamics of industrialism (and postindustrialism), that is, a theory of the creation, destruction, and interconnection of the places in the economic, political, and educational arenas.

Such a conclusion carries with it two sets of programmatic directives.

First, there is no point in ranking empty places (for example, occupations) in terms of their status. The transformation of the social structure (in particular, the occupational structure) can be understood only from the perspective of the *activities* or *functions* associated with sets of empty places. Scales of occupational prestige become irrelevant. Second, the shift of focus to questions of structural change implies a concern "with the ways in which differential class power and social advantage operate in predictable and routine ways, through specifiable social interactions between classes or interest groups, to give shape to determinate social structures" (Coser 1975, p. 694). In other words, attention is directed to the way a social structure conditions the appearance of solidary groups and organizes them into social forces, and how the latter in turn transform (within limits) the economic, political, and educational structures. (Tilly, Tilly, and Tilly [1975] provide a wide-ranging historical analysis of some of the issues involved in such an approach; Przeworski [1976] presents a cogent theoretical formulation.)

Social Origins and Educational Attainment

To illustrate some of the problems I have been alluding to, I will now briefly examine one of Treiman and Terrell's more surprising conclusions: "In both countries education is largely independent of social origins and thus serves mainly as a channel of social mobility rather than an instrument of status maintenance" (p. 577). How do they reconcile such a conclusion with the more conventional view that "the most important determinant of educational attainment is family background" (Jencks et al. 1972, p. 158)? In fact they make no attempt to explain the discrepancy between their own results and the more generally accepted position (about which, of course, they are definitely aware; see p. 576). This is significant, since the paradox can only be resolved by digging beneath the polished statistical analysis and unearthing the assumptions upon which their conclusions rest.

First, the low correlation between father's occupational prestige and educational attainment (.293 for the United States and .335 for Britain) indicates at best that their standardized occupational prestige scale is not a relevant measure of the social origin bias of educational attainment. Thus, in the United States an index of income and occupational status correlates .55 with educational attainment (Jencks et al. 1972, p. 138). There are also other measures of social origin such as parents' education, income, wealth, and class—either a Marxist version (Wright and Perrone 1975) or a Weberian model based on life chances (Bertaux 1976). These have to be explored before concluding that "education is largely independent of social origins."

Second, standardizing their measures of educational attainment obscures as much as it reveals. Whereas Treiman and Terrell adopt a two-dimensional model of the British *educational* system, they do not incorporate findings by Turner and Himmelweit (1974) that Britain also has two systems of *status attainment*—grammar schools do serve as a channel of mobility while secondary modern schools tend not to. (Treiman and Terrell do seem to be aware of such a possibility—see n. 8, p. 571—but claim their sample is too small to do anything about it.) Their treatment of the United States system of education as unidimensional rests on an analysis of *expected* occupations of public school graduates (pp. 580–81). This is hardly satisfactory. Both Bowles and Gintis (1976, chap. 5) and Bidwell and Kasarda (1975) question the conventional view that the educational "output" of the United States school system is unidimensional. Moreover, if variation within schools is so much greater than variation between schools, as is usually supposed, attention should be directed to patterns of internal stratification (Heyns 1974).

Third, low correlations may be a function of the heterogeneity of the sample. If the relationship between father's occupation and educational attainment of son varies according to social class and age of cohort (both father's and son's), then combining the different, but not necessarily weak, relationships inevitably contributes to a lower sample correlation.

Fourth, education *may* be a channel of mobility for the "burgeoning middle classes"—thereby providing a concessional basis for the ideology of equality of opportunity—but at the same time it acts as an instrument of status maintenance for the rich and poor. In other words, the sample correlation may reflect the numerical preponderance of a relatively homogeneous middle stratum. With only nine of the sample having attended public schools (table 2, p. 570) and 14 with a university degree (n. 7, p. 571), Treiman and Terrell inevitably miss the important fact that the various British national elites are dominated by a select group from a common social and economic background who have made their way through certain public schools and Oxbridge (see, for example, Sampson 1965). What is numerically or statistically insignificant is often sociologically central. Nor should it be forgotten that for those with wealth, inheritance is the dominant mode of transmitting status, while education (as a means of social mobility) is relevant only for those without wealth. That is, education means different things to different groups, a fact which is necessarily overlooked in the process of standardization.

Fifth, how much mobility is there? Is mobility confined to movement within specific groups such as classes? Is there a great deal of mobility between neighboring occupations or a little mobility between distant occupations or both? Are there different patterns according to the direction of mobility?

Sixth, the conclusion that education serves as a channel of social mobility is based on a dependency of occupational achievement upon education. This implies that insofar as education acts as an instrument of intergenerational mobility it is also an instrument of intragenerational *im*mobility (Lane 1972). To treat the one in isolation from the other is to mystify the functions of education. Seventh, as I indicated earlier, without invoking a model which takes into account changes in the occupational structure, a sociologically meaningful interpretation of path and correlation coefficients is virtually impossible.

Conclusion

By now the message must be obvious. Most of what is interesting, and also sociologically important, about Britain and the United States escapes the bland homogenizing of linear statistics and standardized scales. Two complex social structures with differentiated nonlinear systems of stratification cannot, with any theoretical meaning, be reduced to timeless regression coefficients (themselves largely meaningless). To do so is a case of "misplaced precision"—the belief that "one can compensate for theoretical weakness by methodological strength" (Coser 1975, p. 692). We have observed how methodological precision can disguise what is theoretically and substantively fanciful. Societies are complex, so we must simplify. But there are simplifications and simplifications. Some are reasonable; some are not.

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TOWARD METHODS FOR A QUANTITATIVE COMPARATIVE SOCIOLOGY: A REPLY TO BURAWOY[1]

In his lengthy commentary on my article with Kermit Terrell comparing the process of status attainment in the United States with that in Great Britain (Treiman and Terrell 1975*a*), Michael Burawoy steals a theme from Coser (1975) and accuses us of emphasizing method over substance to the detriment of an adequate understanding of similarities and differences in the stratification systems of the two societies The issues Burawoy raises are important, although his tendency to invective, errors in logic, and shoddy scholarship detracts from what is serious in his argument. As I see the main points of contention, they reflect aspects of a single fundamental issue: Is a quantitative comparative sociology a sensible endeavor? This can be usefully treated as a series of questions: (1) what is the proper aim of comparative analysis? (2) what are the proper methods? and (3) can elements of social systems be validly compared in abstraction from the unique historical experience of each society involved in the comparison? Although these are not the issues Burawoy explicitly identifies, they seem to me to underlie what is serious and not merely silly in his critique. I therefore propose to address these questions one by one, after which I will respond to his attack upon our conclusion that educational attainment is largely independent of social origins.

The Aim of Comparative Analysis

Burawoy complains that "subtleties which lend a particular system of stratification its specific character and structure are obliterated in the process of standardization. . . . Treiman and Terrell, rather than allowing similarities and differences to emerge from careful study of particular societies, impose uniformity upon them all." If he means the point to be taken generally, he is fundamentally challenging the idea that societies can be compared at all. Comparison inherently involves abstracting par-

[1] This comment has benefited greatly from discussion with Judith Herschman, Patricia Roos, and J. L. P Thompson Its preparation was supported in part by the National Institute of Mental Health (MH 26606, "A Comparative Study of Status Attainment").

ticular dimensions from a whole. One cannot compare apples and oranges, unless one chooses to concentrate on their relative size, the character of their skins and meat, their nutritional value, or other specific traits. Of course, in abstracting specific traits, the *essence* of the whole may be lost. But unless one does so, such objects as apples and oranges remain noncomparable. So it is with societies. If one's purpose is to understand what is unique about particular societies at particular points in their histories, abstraction of specific traits is self-defeating But while this seems to be Burawoy's purpose, as evidenced not only by his remarks quoted here but by his justification for his own comparative analysis (Burawoy 1976, pp. 1053–54), it was not ours. I take a rather different view of the role of comparative analysis in sociology. In my view, we should strive for a discipline which distinguishes three classes of phenomena: what is true of all societies, what systematically covaries across societies, and what is unique to particular societies.

But to do so we must move away from a case-study approach and toward a mode of analysis which treats societies as the unit of observation and studies the pattern of covariation among various aspects of social structure. For example, to settle such issues as whether income inequalities are reduced by a socialist form of economic organization as compared with a capitalist form, we would need to measure income inequality in a comparable way in the two kinds of society. Our only other choice would be to abandon the question on the ground that economic rewards are so intrinsically noncomparable in capitalist and socialist societies that a comparison is impossible in principle. Similarly, to settle the question of whether education plays a more important role in occupational attainment in industrialized than in nonindustrialized societies, we need to be able to measure the connection between educational attainment and occupational attainment in comparable ways across societies or else abandon the question. Burawoy may wish to abandon the question, but I do not. Accordingly, I have been interested in developing methods for effecting systematic cross-societal comparisons. The British-American comparison which Burawoy criticizes is but one step in this direction (see also Treiman 1975; 1977, chaps. 8 and 9).

On Methods of Comparative Analysis

One may, of course, accept the aims of a quantitative comparative sociology without accepting the validity of the methods we used to achieve comparability in our British-American comparison. Throughout his critique Burawoy asserts (but does not demonstrate) that our procedures impose upon the two societies homogeneity which does not exist; in particular, he objects to our use of standardized scales and linear statistics

and to certain restrictions in our samples. Let me address these concerns one by one.

The use of standardized scales.—The advantage of a standardized measurement procedure is obvious—only by using such a procedure can one compare results for different units, in this case societies. But the potential disadvantage is that one may misrepresent or distort the "true" value for one or another of the units being compared. In the present instance, the issue is whether use of a standardized occupational prestige scale distorts comparison of the process of status attainment in the United States and Britain. Burawoy claims that it does. But in what does his claim consist? In no more than the assertion that whereas in Japan "whom you work for is more important than what you do," "in Britain the class to which one belongs is probably a more significant measure of status than one's occupation." This is entirely irrelevant to what Terrell and I were interested in studying—the process by which people acquire education, occupations of particular prestige, and income. Whether or not occupational status is an important attribute by which people judge the social standing of themselves and others (and interestingly, despite Burawoy's implication to the contrary, the available evidence suggests that it is important in both Britain and the United States; see Dore 1973, p. 115; Covello 1974), it is legitimate to study whether the process of occupational attainment is similar or different in the two countries. Then the relevant question becomes whether occupational prestige and the other variables under study, such as education, are properly and validly measured in the two countries. And on this score Burawoy has nothing to say except to brand the prestige scale "ethnocentric." Such pejorative labels notwithstanding, the fact remains that the populations of Britain and the United States evaluate the prestige of occupations in a virtually identical way, which was documented in our original article and which Burawoy himself concedes. What, then, is ethnocentric about the scale?

Similarly, the validity of our educational attainment scale turns on whether we have captured the *occupationally relevant* variance in education, for the issue at hand is whether the strength of the connection between educational attainment and occupational attainment is similar or different in the two societies. Rather than impose a common measurement procedure on the two societies—which is what sole reliance upon school-leaving age would have amounted to—we sought a means of representing the multidimensional character of the British educational system by developing a scale which represents the effect of *type* of schooling as well as *amount* of schooling on occupational attainment. The importance of having done this is obvious from a comparison of the school-leaving-age variable and the effect-proportional scale. Whereas use of the former implies that the correlation between educational and occupational attain-

ment is weaker in Britain than in the United States, use of the latter implies correctly that the correlation is slightly stronger in Britain.

Despite the evidence just cited, Burawoy seems to be under the mistaken impression that when things are measured with the same instrument they always appear to be the same size. It is an elementary point that without comparability of measurement it is impossible to discover *either* differences *or* similarities between societies (Treiman 1975). Contrary to Burawoy's blanket characterization of our analysis as imposing "homogeneity upon heterogeneous social structures," our paper sought to establish precisely how the systems differ: Great Britain has a multidimensional educational system in which the type of schooling as well as the amount of schooling is crucial for occupational and economic achievement, whereas the United States has a unidimensional educational system in which only the amount of schooling is important However, despite these very real differences, the strength of the effect of educational attainment on subsequent occupational status is essentially the same in the two societies. On the other hand, the connection between father's and son's occupational status is stronger in Britain because factors other than schooling are more important in Britain, and the dependence of income on educational and occupational status is much stronger in Britain. Thus, overall, Britain has a somewhat more rigid stratification system.

The choice of comparison groups.—As Burawoy correctly points out, the conclusions above pertain only to white male heads of households aged 25–64 in the civilian labor force of each society. He challenges our exclusion of female, black, young, and old workers, on the one hand, and those not in the labor force, on the other hand, on the ground that their differential representation and different patterns of status attainment are what give the two societies their distinctiveness. Despite Burawoy's rather silly attempt to brand us as elitists (white males are not "more significant" than women and blacks; they do, however, represent the majority of the *labor force* in both countries), his critique does raise two important issues: (1) should studies of status attainment be restricted to the "economically active" population? and (2) even if they should, are further restrictions of the population legitimate when making intersocietal comparisons? Of the two, I regard the latter issue as by far the more serious.

With respect to the first issue, Burawoy argues that since housewives, students, retirees, etc., "occupy significant places in the social structure" their activities should be regarded as "occupations" and included in the analysis of status attainment. I agree with Burawoy that "ideology should not get the better of sociological sense," but I suggest that he, not I, is the victim of this malady in his insistence that the economically active

and noneconomically active population be included together in a single analysis. For these are qualitatively different groups. The economically active population operates in a labor market in which there is a direct exchange of labor for income and in which the allocation of persons to positions reflects the simultaneous attempts of both workers and employers to maximize the returns on their investments. With respect to such a system it makes perfectly good sense to investigate the rates of conversion of education into prestigious jobs and of prestigious jobs into income and also to investigate the extent of advantage created by parental status. For the remainder of the population this sort of investigation doesn't make any sense One cannot very well study occupational attainment (in the ordinary sense of the term, referring to market labor) or income for that portion of the population which does not have an occupation which produces income. Burawoy seems to wish that we had conducted some other study, concerned with the status attainment of the entire population. But nowhere does he make clear what he means by "status attainment," other than what we, following a highly conventional usage, quite obviously meant

With respect to the second issue, Burawoy is quite correct, as I have come to feel since writing the original article: one should not arbitrarily exclude elements of the population ostensibly under study In the present instance, this means that it would have been preferable to compare the entire labor force of the United States and Britain without restrictions based on sex, race, age, or head-of-household position, yet at the same time to take account of the impact of these variables on the status-attainment process.

In principle this would be easy enough to do by means of a conventional regression analysis including dummy variables and interaction terms. Unfortunately, what is straightforward in principle is a good deal more complex in practice, for several reasons. First, the enormous cost of carrying out comparative research ordinarily requires the comparative analyst to rely upon available data, which means that often variables of interest are not available. This was the reason we restricted our comparison to male heads of households—no data for females were included in the United States sample, and income data were available only for household heads in the British sample.

Second, the limited size of most representative sample surveys of general populations (typically less than 2,000 cases) often has the consequence that members of substantively important but numerically small subgroups are not included in the sample in sufficient numbers to permit reliable estimates. For example, there were not enough grammar-school-educated men in the British sample to sustain a separate analysis of that group, despite its importance as an incipient elite.

Third, the inclusion of certain groups can create as much distortion and bias as their exclusion. For example, age 25 was chosen as a lower limit in order to avoid including only a biased sample of younger people —those who had already finished their education and were working. The lower the age chosen as a cutoff, the greater the proportion of the age group excluded and the more downwardly biased the estimate of the correlation between education and subsequent status attainment. This problem, which is endemic to all status-attainment studies, is compounded in cross-national comparisons by societal differences in the age at which people ordinarily begin working.

Finally, even when large samples containing all the pertinent variables are available, the complexity of the analysis usually necessitates some simplification. Although it would be possible in principle to estimate a regression equation which included dummy variables for race, sex, age, size of city, region, marital status, number of children, and all other variables known to affect the status attainment process, plus all the appropriate interaction terms, the result would be extremely difficult to interpret. For this reason, most studies—not only of status attainment but of other phenomena as well—concentrate on a limited set of issues, and by the gradual accretion of knowledge the field makes progress. In the present instance, we were attempting the *first* comparison of status attainment in the United States and Britain which made a concerted attempt to achieve comparability of measurement; and at the time we prepared the original draft, for the 1972 ASA meetings, there had been *no* studies comparing the status attainment of men and women, either in the United States or anywhere else (in fact Terrell and I published the first such study, 1975*b*, originally prepared for the 1973 ASA meetings). It is no more reasonable for Burawoy to insist that we encompass all subgroup variations in the status-attainment process in a single study than it would be for me to insist that he not undertake a study of migrant labor except in conjunction with a simultaneous study of non-migrant labor on the ground that the one system does not exist independently of the other (see Burawoy 1976).[2]

The use of linear statistics.—Once again, Burawoy has turned an important issue on its head. There can be no disagreement with the assertion that "we must . . . develop models which directly mirror our understanding (however crude it may be) of social processes." But there

[2] Were I to do the analysis over, the one change I would clearly make would be to include nonwhites However, since nonwhites represent a negligible proportion of the British population and little more than 10% of the American population, their inclusion would hardly have altered our conclusions. For a detailed comparison of the process of status attainment of whites and nonwhites in the United States, see Duncan (1968*a*) and Featherman and Hauser (1976).

is nothing inherent in either regression analysis or Goodman's hierarchical models which precludes this. Contrary to Burawoy's understanding, regression analysis can handle discontinuities and nonlinear functions through the appropriate use of nonlinear transformations, dummy variables, and interaction terms. Likewise, Goodman's procedures are not limited to qualitative data, although they are designed to handle such data in an elegant way; quantitative data can always be converted to categories, although unless there are important discontinuities it is not clear why one would ever want to do so. Taken together, regression analysis and Goodman's models constitute a very powerful set of methods for representing *whatever* model one deems appropriate.[8]

The issue is not the choice of statistical procedures but the assumptions one makes about the social world. Burawoy accuses us of "eliminating nonlinearities and discontinuities or . . . turning them into linearities and continuities by methodological fiat," thereby effecting "an artificial convergence between status attainment in Britain and in the United States." As I indicated at length above, our procedures were designed explicitly to represent properly the "nonlinearities and discontinuities" in the educational systems of the two countries. Are there other nonlinearities and discontinuities which we have failed to heed? Burawoy has neglected to identify any.

Placing Status Attainment in Structural Context

In the second part of his critique, Burawoy makes the bold assertion "that the interpretation of status attainment can be undertaken only with

[8] Incidentally, in his discussion of "sociologically sensible baseline models," Burawoy draws a false distinction between the two techniques. Regression analysis is not limited to "sociologically absurd" comparisons, and Goodman's models do not "invariably analyze . . . discrepancies between observed values and . . . a model which makes sociological sense." Both procedures permit a variety of comparisons. Typically, the Goodman procedure involves comparison of observed values with successively more complicated models, starting with the clearly sociologically absurd model of equal cell entries, moving to the nearly as absurd model of statistical independence between variables, and so on (Davis 1974). This is not to denigrate the technique—the point of the procedure is to find the simplest model which fits the data reasonably well. Regression procedures have a different purpose, to estimate the form and strength of connections between variables Hence, a sociologically sensible interpretation of regression coefficients typically involves comparison with other regression coefficients. Such comparisons can be of two kinds: within populations and between populations. For example, out data (table 4) showed that in both the United States and Britain the effect of educational attainment on occupational attainment is far stronger than the effect of father's occupation (the ratio of the standardized coefficients is nearly four to one in Britain and over five to one in the United States). But the income payoff on occupational status is much greater in the United States, as can be seen by a comparison of the metric coefficients across the two countries

reference to the historically specific social structure in which it occurs." This leads him to conclude that we ought first to study the emergence of the specific structures in which the status-attainment process is embedded and then study the effect of structure on process. But this conception of how to proceed is based on a number of untenable theoretical assumptions.

Does structure precede process?—Burawoy clearly considers the distribution of occupational positions, educational achievements, and (presumably) incomes to be determined by forces exogenous to the status-attainment process. This is what I take him to mean by "the pattern of 'empty places' between which individuals move" and "the patterns of empty places which define the educational and occupational structures." In support of this conception he invokes White's (1970) study of the movement of Episcopal clergy through positions in the church hierarchy. Yet, outside of churches and other rigidly structured organizations, it is probably nearly as likely that positions are created or abolished in response to the availability or unavailability of individuals to fill them as the other way around. Public works programs are created to reduce unemployment, places are found for relatives who need jobs, processes are automated in the face of labor shortages, and so on (see Treiman 1970, pp. 222–24, for a fuller discussion of the ways the mobility process itself can alter the occupational structure). Similarly, except in highly centralized economies such as those of Eastern Europe, the number of openings in schools and degrees awarded is not rigidly fixed and is, in fact, highly responsive to political pressures to expand. Hence, there is no particular reason to treat the structure as predetermined and the status-attainment process as occurring within a fixed structure, as Burawoy would have us do. Instead, we should look for ways to study the quite complex interaction between structure and process. But while we search for such ways, we need not abandon attempts to study societal variation in the status-attainment process without regard to structural variations. Indeed, only when the results of such studies as we conducted are available for a large number of societies will it be possible to carry out a systematic investigation of the way the status-attainment process is affected by (and affects) the status structure.

Does structure affect process in the United States and Britain?—While a cross-sectional investigation of social structural variations in the process of status attainment must await the accumulation of data for a larger number of societies—a task in which I am currently engaged (see Treiman and Kelley 1974)—it is possible to inquire whether changes in structure over time produce concomitant changes in process. Burawoy takes us to task for not doing this, not understanding that it has already

been done. The pertinent evidence is of two kinds. If, despite the reservations expressed above, we accept the conventional view that the pattern of mobility is constrained by the occupational structure, we can interpret the work of Hauser et al. (1975) on the United States and of Hope (1974) on Britain as showing that observed changes in the *pattern* of mobility during the twentieth century can be entirely accounted for by changes in the occupational structure.[4] That is, drawing the traditional distinction between *structural* and *exchange* mobility (mobility induced by change in the occupational structure vs. mobility occurring independently of structural change), we can conclude that the amount of the latter has been constant throughout this century. This implies the strong likelihood that despite the upward shift in the occupational structure during this period, the correlation between father's and son's occupational status has remained constant, and that proves to be more or less true, at least in the United States (Duncan 1968*b*, tables 1 and 2 and pertinent discussion). Further, in both the United States and Britain the intercorrelations among those variables measured at a fixed point in the respondent's life (e.g., father's occupational status, education, and status of first job) are essentially invariant across 10-year age cohorts within the current labor force (Duncan 1968*b*, tables 3 and 4; Psacharopoulos 1976, appendix table A). Therefore we are perfectly justified in concluding that the relationships we demonstrated are invariant with respect to cohort, despite Burawoy's statement to the contrary in his section headed "Cohort analysis and changes in the social structure." Moreover, it makes sense to consider only such changes in social structure as are pertinent to the issue under study. Hence, Burawoy's preoccupation with the 1944 reform of the British educational system is puzzling. It is hard to know exactly at what point in the educational career the reforms might be expected to have had an impact, but if we take as a clearly affected group those who first entered school in 1944 or later we observe that in 1963, the date of our British survey, such men were at most 24 years old. How then could the 1944 reform have affected the process of status attainment in our sample, which was restricted for analytic purposes to those 25 and older?

4 Burawoy demonstrates that he is seriously confused when he fails to distinguish between the *amount* of mobility induced by changes in the occupational structure ("structural mobility") and *changes* in the amount (or pattern) of mobility. Thus he cites Duncan (1966, pp. 70–76) as showing that "83% of intergenerational mobility can be attributed to changes in the occupational structure." But this is not what Duncan showed at all; rather, he concluded (pp. 74–75) that 83% of the difference in the *pattern* of mobility in Indianapolis between 1910 and 1940 could be accounted for by shifts in the occupational distributions of fathers and sons. This same confusion permeates and renders invalid Burawoy's entire discussion of the relationship between structure and process.

Social Origins and Educational Attainment

In his final section Burawoy objects to our conclusion that "in both countries education is largely independent of social origins and thus serves mainly as a channel of social mobility rather than an instrument of status maintenance" (Treiman and Terrell 1975*a*, p. 577). He begins by expressing concern regarding the apparent discrepancy between our conclusion and what he labels the more conventional view that "the most important determinant of educational attainment is family background" (Jencks et al. 1972, p. 158). Unfortunately, Jencks is internally inconsistent—the quoted assertion is at variance with the path models on page 339 of Jencks's monograph, which show IQ to be a more important determinant of educational attainment than the combination of father's occupation and father's education. But even if we accept the quoted phrase as the "conventional view" there is no contradiction—the most important (measured) determinant of education could be family background without the influence of family background being very large relative to the residual or unmeasured determinants of educational attainment, which by definition must be uncorrelated with the measured family background variables. And insofar as educational attainment is determined by factors uncorrelated with family background variables, education necessarily serves as a mechanism of mobility rather than one of status maintenance, since what we mean by mobility is status attainment independent of origin status and we have already established the strong connection between education and occupational attainment.

Of the several numbered points Burawoy makes in the course of his critique, the first two merit a serious response. With respect to each I will first indicate Burawoy's criticism and then present my response.

(1) *Criticism*: The low correlation between father's occupation and educational attainment betrays a flaw in the occupational scale; moreover, by not including other social origin variables we understate the role of social origins. *Response*: First, in contrasting our correlation of .29 between father's occupational prestige and educational attainment with Jencks's correlation of .55 between educational attainment and an index of parental income and occupational status, Burawoy misquotes Jencks on two counts. he represents as an actual correlation what Jencks clearly labels as a hypothetical figure, a guess about the probable size of the correlation if such an index were available, and he fails to report that Jencks is dealing with correlations corrected for attenuation, which are almost always inflated relative to observed correlations. The appropriate comparison, given the point Burawoy wished to make, is between our correlation and the correlation of .42 between father's occupational status measured with Duncan's Socioeconomic Index and educational attainment,

reported by Jencks on page 322. The difference of .13 between our correlation and that reported by Jencks is real, since they are both based on the same data set (although with minor differences in the definitions of the samples for which the correlations were computed). This difference reflects a tendency for prestige scales to yield lower correlations than does Duncan's scale between father's occupational status and other variables, although not between respondent's occupation and other variables (Treiman 1977, table 9.3). But the basic conclusion is hardly an artifact of the scale. Blau and Duncan reached exactly the same conclusion—that education is mainly a mechanism of mobility—in their original analysis of these data, which made use of the Duncan scale (Blau and Duncan 1967, pp. 200–201).

Second, additional social origin variables were not included because they were not available in the British data. But, if they had been available, their inclusion would not likely have altered our fundamental conclusion. This is because, as I have already pointed out above, the addition of variables to a model can increase the explained variance in the dependent variable only to the extent that the new variables are uncorrelated with those already in the model. Since variables representing the social status of family of origin tend to be positively correlated, their effects on educational attainment are likely to be highly redundant. Thus, for example, Sewell and Hauser (1975, tables 3.8, 3.11, and 3.13) explain 8.5% of the variance in the educational attainment of male Wisconsin high school graduates on the basis of father's occupational status (Duncan scored), 13.0% on the basis of father's occupation and father's education, 14.1% on the basis of these variables plus father's income, and 15.5% on the basis of the preceding variables plus mother's education and income. Similarly, in an exercise Hauser and I conducted on the Blau and Duncan data, the addition of a hypothetical variable representing parental income increased the explained variance in educational attainment by at most 4% over what was explained by father's education and occupation (Treiman and Hauser 1976, tables 2 and 4). For this reason it should come as no surprise that none of the major status attainment studies in the United States accounts for as much as 30% of the variance in educational attainment on the basis of family background even when such variables as number of siblings and intact family are introduced (in addition to the studies just cited, see B. Duncan 1966; Alexander and Eckland 1974; and McClendon 1976). This seems to me quite consistent with the claim that educational attainment is largely independent of social origins.

(2) *Criticism*: The use of a single summary measure for educational attainment misrepresents the multidimensional character of the educational system in both countries. *Response*: With respect to the United

States I can only refer Burawoy once again to the appendix to our original article, which shows that type of schooling makes no difference in occupational attainment beyond what is accounted for by amount of schooling. Contrary to Burawoy's assertion, the claim does not rest upon any single study but upon data from five different studies With respect to Britain, Burawoy is correct that the impact of a given amount of grammar schooling is different from (greater than) that of an equal amount of secondary modern schooling, but he fails to appreciate that this difference is captured by the effect-proportional scale—indeed, that it was the very purpose of that procedure to correctly represent this sort of difference.

The remainder of Burawoy's points refer mostly to issues which I have already dealt with above and hence need not detain us further here.

Concluding Remarks

At the beginning of this comment I suggested that the fundamental point of contention between Burawoy and myself is whether a quantitative comparative sociology is a sensible endeavor. I think that it is, if our purpose is to understand the general properties of social systems rather than the particular features of particular societies. To distinguish the general from the particular requires precisely comparable data for a large number of cases, and I am currently engaged in research which ultimately will yield estimates of the parameters of a model of status attainment for more than 40 societies. But to achieve cross-cultural comparability of measurement is a difficult task, which has not been seriously attended to in past comparative stratification research. Our comparison of status attainment in the United States and Britain was intended to serve as a vehicle for the exploration of strategies for effecting such comparisons. It also served to provide the first valid comparison of the two systems of status attainment and hence has a substantive as well as a methodological interest.

Burawoy, of course, contends that important substantive considerations have been ignored because of our preoccupation with methods. Indeed, in his introductory paragraphs he goes so far as to characterize us as engaging in atheoretical statistical manipulation. However, it should be obvious by now that our entire analysis was predicated upon a concern for representing properly the special features of each of the two systems, but in such a way that they could be compared. Moreover, far from being atheoretical, we have been quite explicit about our theoretical assumptions. The real issue is not the presence or absence of theory but the nature of our assumptions. Burawoy obviously disagrees with us in several respects, but he has neither articulated the precise nature of his

objections nor proposed any clear basis for alternative formulations; instead he has relied upon the technique of categorical assertion. Two final examples will illustrate my point.

First, we assumed occupation to be central to the stratification system, both as a resource which helps to determine the life chances of oneself and one's children and as a source of intrinsic (prestige) and extrinsic (income) rewards. Moreover, we assumed that occupations are hierarchically ordered and that it is sensible to measure both the resource and reward aspects of occupations by their relative positions in the hierarchy and to ask to what extent and by what means occupational standing is preserved across generations. These assumptions were not made mindlessly; they derive from a great deal of research on occupational stratification and mobility (e.g , Duncan 1961; Klatsky and Hodge 1971; Treiman 1977). Burawoy categorically rejects occupational ranking scales, but without any argument against them or justification of his position What conception of how people are allocated to occupational positions leads him to this stance? Or does he reject altogether the study of occupational attainment? At various points he calls for a "class analysis," but he never makes clear what relationship, if any, exists between occupation and class.

Second, Burawoy asserts categorically and without apparent recognition of alternative theoretical formulations that the distribution of occupational roles is prior to the mobility process As I have shown above, this need not be so—there is good reason to conceptualize the relationship between structure and process quite differently, regarding the distribution of sons' occupations as determined by the distribution of fathers' occupations plus a particular pattern of mobility. But even if one accepts Burawoy's assertion, there is nothing in it which invalidates a regression analysis of status attainment. He has failed to show precisely how changes in occupational structure might be expected to affect the occupational attainment process. I do not believe that the connection is at all straightforward or obvious and hence consider it Burawoy's obligation to document his claims with something more than categorical assertions.

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Review Essay

Lineages of the Capitalist State*

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I

Suppose you want to build a general theory of social change. Such an enterprise, if it is to succeed, must proceed through two distinct stages; the first is morphological, the second explanatory. First, all known social formations, both past and present, must somehow be classified and thereby reduced to a limited number of elementary forms of social organization. In principle, the resulting classificatory scheme should be comprehensive, so as to incorporate all observed cases, yet parsimonious as well, so that simple "laws" governing evolutionary dynamics might thereby come to the fore, if indeed such laws can be conceived to hold. Further, there must be a vertical dimension to this scheme, roughly corresponding to temporality, enabling some—though not all—of these forms of social organization to be ordered hierarchically into more and less advanced types. Once this formidable task is completed, it becomes possible to contemplate the second, explanatory, stage. Here the question, though not the answer, is simple: How can one account for the particular historical transitions from less to more advanced forms of social organization?

The formal resemblance of the problem of societal evolution to that of the evolution of biological species has long been recognized, as have the great differences between these two types of processes. It is the magnitude of the differences which is doubtless responsible for the fact that our understanding of the latter process is so much more developed than our understanding of the former. The morphological basis of biological evolution was laid in the 18th century by the Swedish botanist Linnaeus. By the mid-19th century, enough conceptual progress and empirical observation had been made to inspire a theoretical synthesis.

The problem of societal evolution has proven more intractable. Sociology still awaits its Linnaeus. Those who feel that the history of biology may serve as a partial model for the development of scientific sociology must be very pessimistic about this: The gap between Linnaeus and Watson-Crick is a very wide one indeed. But mechanical as Linnaeus's con-

* *Passages from Antiquity to Feudalism.* By Perry Anderson. London: New Left Books, 1974 Pp. 304. £5 00. Humanities Press, New York. $15 00.

Lineages of the Absolutist State. By Perry Anderson. London: New Left Books, 1974. Pp 573. £8.50. Distributed in the United States by Humanities Press, New York. $25.00.

tribution was, it was also indispensable. Simply stated, there can be no general theory of social change in the absence of an adequate morphology of social forms. However, as of yet there is little agreement on how such a morphology might be constructed. Perhaps for this very reason the most important debates about social change revolve around questions of morphology. Precisely how can the elementary forms of social organization be determined?

This is a particularly troublesome conceptual problem for sociology because the boundaries of social units are by no means self-evident. Social formations do not have actual membranes, like the epidermis of vertebrates, which allow them to be distinguished from one another. Hence, the choice of the appropriate unit of analysis in the study of social change necessarily requires theoretical justification.

Since social science has always been practiced by individuals living within politically defined units, states of one sort or another, it is easy to understand why the political boundary has come to be perceived as analogous to the epidermal membrane separating biological organisms. Social units of this type can be conceived to be like organisms. Each possesses a political structure, including the state apparatus, which is a source of imperatives; a cultural structure, through which commands can be passed and feedback relayed; and an economic structure, which provides energy for the unit as a whole. All of these component systems operate for the most part within a bounded territory. Theories which locate the causes of development within social units defined by political boundaries may be termed "ontogenetic" (Hechter 1975*a*, p. 217).

But it is obvious that each state is also a unique individual, having distinctive institutions and customs in all spheres of its domain: here there is agriculture, there industry; here dictatorship, there democracy; here aristocrats, there bourgeoisie; here common law, there Roman law; here scones, there *croissants*. Lists of such differences between any two social forms can be endless. Each of the classical sociological theories specified a certain range of institutional differences as basic for the classification of social forms into discrete types. Some of these theories emphasized dominant ideologies, others stressed political, legal, or economic institutions as the bases for classification. By and large, however, these 19th-century morphologies—many of which reduced to simple dichotomies or trichotomies—were constructed in an ad hoc manner. Although they may still be mentioned in courses on the history of sociological theory, many enterprising students quickly put them out of mind. And rightly so. For, with perhaps one exception, all of these categories have by now died quiet deaths.

For reasons both intéllectual and political, Marxism has stubbornly refused to die. Marx's typological scheme, based upon his notion of the "mode of production," continues to be the subject of much discussion and debate, particularly in Western Europe, despite (or because of?) the concept's evident ambiguity. A simplistic definition of the mode of production is given in the preface to *A Contribution to the Critique of Polit-*

ical Economy (1859): "In the social production which men carry on they enter into definite relations that are indispensible and independent of their will; these relations of production correspond to a definite state of development of their material powers of production. The sum total of these relations of production constitutes the economic structure of society —the real foundation, on which rise legal and political superstructures and to which correspond definite forms of social consciousness. The mode of production in material life determines the general character of the social, political, and spiritual processes of life" (Marx 1959, p. 43). This definition is belied, however, by Marx's more subtle empirical analyses elsewhere (particularly in the *18th Brumaire of Louis Napoleon* and the *Grundrisse,* which treat various superstructural factors as having independent significance). Further, while the theory of historical materialism assumed some kind of progression of modes of production in world history—including such types as the primitive communist, the ancient, the Germanic, the so-called Asiatic,[1] the feudal, and finally the capitalist—Marx and Engels devoted most of their efforts to analyzing capitalism. Inevitably, their discussion of precapitalist modes of production was truncated. More recent scholarship within the Marxist framework has attempted to redress this imbalance. Much of this work has dealt with the problem of transition from one mode of production to another, particularly from feudalism to capitalism (Dobb 1946; Sweezy 1950; Hobsbawm 1964; Hindess and Hirst 1975). Yet, even in these discussions the concept of mode of production is left somewhat cloudy.

Still, a consensus on the concept of the mode of production appears to be emerging in this literature. It may be stated thus: A mode of production combines both forces and relations of production The relations of production refer to the means by which surplus labor is appropriated. Surplus labor is appropriated in all modes of production, but such appropriation may take one of several specific forms. Some contemporary Marxist theorists have been at pains to eradicate all forms of vulgar determinism from their conceptual frameworks.[2] They have begun to insist that the superstructure of any given social formation cannot be deduced from knowledge of its mode of production alone (Althusser 1970, pt. 3). The mode of production serves merely to limit the range of possible variation in the economic, political, and cultural levels of given social formations. Whereas modes of production survive over time if they are "reproduced" (in various unspecified ways), the presence of a particular mode of production is not sufficient to secure the reproduction of its conditions of

[1] The so-called Asiatic mode of production—once the subject of furious debate among Marxists and ex-Marxists alike—has now apparently bitten the dust, and none too soon For two very different autopsies of this tautological concept, see Anderson (1974*b*, pp. 462–549) and Hindess and Hirst (1975, pp. 178–220).

[2] The lineages of this tendency lie deep within the Marxian tradition See, for example, Friedrich Engels's letter to Joseph Bloch (1968, pp. 692–93) and Antonio Gramsci (1971).

existence (Hindess and Hirst 1975, p. 15). If it were, there could obviously be no social change.

Thus many clarifications (or revisions) of Marxism tend to downplay structural determinist versions, thereby asserting the autonomy of some factors previously considered superstructural. For example, in *Lineages of the Absolutist State,* one of the volumes to be considered in this essay, Perry Anderson states (pp. 403–4):

> Capitalism is the first mode of production in history in which the means whereby the surplus is pumped out of the direct producer is "purely" economic in form—the wage contract: the equal exchange between free agents which reproduces, hourly and daily, inequality and oppression All other previous modes of exploitation operate through extra-economic sanctions—kin, customary, religious, legal, or political It is therefore on principle always impossible to read them off from economic relations as such The "superstructures" of kinship, religion, law or the state necessarily enter [*sic*] into the constitutive structure of the mode of production in pre-capitalist social formations . In consequence, pre-capitalist modes of production cannot be defined *except* via their political, legal and ideological superstructures, since these are what determine the type of extra-economic coercion that specifies them

In their insistence on the determinative character of certain superstructural forms, Marxists like Anderson have begun to move toward a position of theoretical circumspection approaching that of Max Weber. Such a move has two consequences. First, the more attention is paid to differences of superstructure between social formations, the more any causal analysis of change will become historicist, and, incidentally, not so distinctive from more conventional "bourgeois" analyses. Second, since superstructural variables (for example, ideologies) are frequently more difficult to operationalize than infrastructural ones (for example, urbanization), this has the effect of making the new versions of Marxist theory less susceptible to empirical verification.

I will argue that these particular conceptual refinements leave these recent writers in the orthodox Marxist tradition wedded even more firmly than were their illustrious forbears to the ontogenetic perspective on social change. Perry Anderson is quite explicit on the primacy of the sovereign state as the key unit of analysis: "One of the basic axioms of historical materialism [is] that secular struggle between classes is ultimately resolved at the *political*—not at the economic or cultural—level of society. In other words, it is the construction and destruction of States which seal the basic shifts in the relations of production, so long as classes subsist" (*Lineages,* p. 11; unless otherwise cited, all references in this essay are to Anderson).

At present the most significant debate about the development of capitalism revolves around the issue of ontogenesis. The explicit critique of ontogenetic theories of capitalist development was begun by Andre Gunder

Frank (1967, pp. 221–42)[3] and has been brought to a new level of sophistication by Immanuel Wallerstein (1974*a*, 1974*b*). Wallerstein has argued repeatedly that the only meaningful social unit in the capitalist world is the world economy. His approach emphasizes that the appropriation of surplus value occurs not only as a function of the relations of production within any given social formation, but also as a function of the unequal relations of social formations linked together in a world economy.[4] He claims further that the capitalist world economy became stabilized around 1640. Thereafter, all states within it—no matter how their methods of labor control may have differed (from wage labor to serfdom), no matter how their political or ideological superstructures may have diverged (from absolute monarchy to bourgeois democracy)—must be considered to be interdependent parts of the capitalist world system.

Since Wallerstein has not discussed the precapitalist era in great detail, it is not clear whether his analysis of that period would necessarily conflict with Anderson's. But on one point, the specification of the mode of production in the area of absolutism, there is a clear conflict between the two approaches. Anderson—keyed to the superstructural differences between industrial capitalist states and the absolute monarchies of the 17th and 18th centuries—insists that absolutism represents the last stage of feudalism. Wallerstein—focusing on the structure of surplus appropriation in the world economy—notices the fundamental similarity of this 17th-century "structure" to its 20th-century analogue. For him, absolutism occurs during the first stage of capitalism. The issue is not merely a matter of labeling; rather, it is a debate about the conceptual scheme most appropriate for the analysis of capitalism.

This essay will analyze and evaluate Anderson's approach in some detail and then compare it with Wallerstein's. It will be seen that in some respects Anderson's ontogenetic and Wallerstein's world-systems analyses essentially complement one another, while in others a choice must be made between them. In particular, Anderson's study offers a workable, although hardly a final, analysis of the decay of ancient civilization. But its greatest merit lies in its compelling explanation of the pattern of structural differences occurring in Europe before the age of absolutism. This offers a solution to an important problem which Wallerstein leaves unresolved in his own work. However, Anderson is much less successful than Wallerstein in accounting for the divergent development of eastern and western Europe following the 17th century. Here the ontogenetic approach runs into great difficulty, whereas the world-systems perspective enters into its own.

[3] Frank's position has been criticized by Laclau (1971).

[4] The precise mechanism of this appropriation of surplus is, however, hardly specified —let alone analyzed—in Wallerstein's account Indeed, this remains a critical weakness of most studies of development carried out from the perspective of dependency

II

In his major study (of which two volumes, *Passages from Antiquity to Feudalism* and *Lineages of the Absolutist State*, have appeared, and a third is in preparation) Anderson seeks to explain why industrial capitalism developed endogenously only in certain parts of western Europe and its overseas settlements. Since the endpoint of this evolutionary process is known, his method is to trace back the history of these particular social formations so that commonalities of their modes of production will be laid bare. If a case can be made that these social formations alone, for whatever reasons, developed a distinctive combination of modes of production, this problem can in principle be solved. Obviously, in such an exercise everything rides on how the mode of production is conceptualized.

In this long retrospective view, feudalism—that is, the historically specific feudalism described by Marc Bloch—is the most dynamic of the precapitalist modes of production, because it was precisely the most classically feudal social formations which later became centers of industrial capitalism. Hence, Anderson is forced by the logic of his method to examine the preconditions of the feudal mode of production. The starting point of his analysis is the ancient world.

Graeco-Roman civilization, from which the concept of the ancient mode of production is derived, was characterized by a particular relationship between town and country. The outstanding intellectual achievements of its cities took place in the absence of an advanced urban economy. Production occurred almost exclusively in the countryside. The Greek and Roman cities were never predominantly centers of manufactures or even of trade; rather, they were urban "congeries of landowners." The wealth of the classical cities derived principally from the production and exchange of staples (corn, oil, and wine) produced in rural hinterlands. Technique was simple. The profitability of this mode of production depended largely upon the volume of trade. Because of limitations of transport technology, any substantial trade involving staples had to be carried out over water. Classical civilization was, therefore, to a large extent geographically predetermined (for the classical analysis of Mediterranean geography, see Braudel [1972]). The Mediterranean is the largest inland sea on the earth's surface, and long-distance transport could thrive in its relatively sheltered waters.

The mode of production which emerged there was based upon the institution of slavery as its linchpin (indeed, Anderson refers to it as the slave mode of production). While slavery had existed prior to the ancient civilizations, it had always been an "inessential" part of earlier modes of production. Although slave labor was never predominant in ancient history as a whole, the periods of greatest material and cultural achievement were marked by the massive and general presence of slavery. The Greek city-states were the first to make slavery a predominant feature of a mode of production by utilizing slaves extensively in mining, agriculture, and crafts. The institution of slavery had profound consequences in all aspects

of social life. In the polity, it gave an economic basis for the evolution of citizenship: "Hellenic liberty and slavery were indivisible: each was the structural condition of the other, in a dyadic system which had no precedent or equivalent in the social hierarchies of the Near Eastern Empires, ignorant alike of either the notion of free citizenship or servile property" (*Passages*, p. 23).

But slavery was also responsible for the principal contradiction of the ancient mode of production. The availability of slaves reduced incentives to develop labor-saving technological improvements. It thus had the effect of paralyzing productivity in agriculture and industry. This tends to explain why the intellectual achievements of the classical civilizations were often skewed toward the theoretical and away from the practical level: "Once manual labor became deeply associated with the loss of liberty, there was no free social rationale for invention. The stifling effects of slavery on technique lay . . . not so much in a direct extra-economic causality, although this was important in its own right, as in the mediate social ideology which enveloped the totality of manual work in the classical world, contaminating hired and even independent labor with the stigma of debasement" (*Passages*, p. 27).

This contradiction was most evident in the Roman Empire, which carried the logic of the ancient mode of production to a higher stage than had the Hellenistic world. Two decisive innovations, one economic, the other legal, helped to spur Roman success. Roman material prosperity was enhanced by the establishment of slave latifundia in the western part of Europe. The latifundia brought significant economies of large scale to the agricultural sector for the first time in ancient history. Overland trade was stimulated by a remarkably effective network of roads. Regional economic specialization was extended. The resulting increases in production made possible a larger and more secure imperial domain.

But this interregional trade was also encouraged by the development of Roman civil law, particularly by the invention of absolute, unqualified private property rights—*dominum ex jure Quiritium*. "The Roman law of property, of which an extremely substantial sector was naturally devoted to ownership of slaves, represented the pristine conceptual distillation of the commercialized production and exchange of commodities within an enlarged State system, which Roman imperialism had made possible" (*Passages*, p. 66).

These refinements in the ancient mode of production made Roman prosperity particularly dependent upon a bountiful supply of slave manpower. After the Empire reached its expansionary limits—once the lands beyond its frontiers were no longer fertile lowlands suited to large-scale agricultural production—Rome's decline was prefigured. The end of expansion soon led to a decrease in the supply of slave labor, and the price of slaves rose correspondingly. The fact that slaves were a perishable capital investment for landowners meant that worker mortality had to be completely written off by the landowners—unlike in a system of wage labor. Further, efforts to replenish the supply of slave manpower through

slave breeding were apparently not very successful. Fertility among the slave population was adversely affected by a skewed sex ratio: female slaves, used primarily for household tasks, were far less numerous than males. While the price of slave labor was steadily escalating, so too were the costs of bureaucratic administration, both civil and ecclesiastical. Fiscal and military crisis was the inevitable product of these trends.

This is certainly a plausible argument, but one need not be a classical scholar to see its limitations. Let us assume that Anderson is justified in his neglect of factors such as soil exhaustion, or climatological variation, or increased mortality—which, while in a strict sense exogenous to the ancient mode of production, might well have contributed to the sapping of Roman strength. The major problem with his analysis is that it cannot easily explain why technological development was not spurred once the cost of slave labor began its precipitous rise. Rome had, in fact, developed some notable improvements, for example the rotary mill, but apparently these were not utilized effectively even throughout the western part of the Empire. This attests to a curious situation indeed. Why should the Romans' antipathy toward manual labor and their resulting disinclination to develop labor-saving techniques have persisted despite crucial changes in the very process of production? In such an argument, part of the superstructure, the ideology of the ancient mode of production, is not only autonomous from the material base, but actually becomes determinative of production. Why this should be so Anderson never makes clear.[5]

[5] Elsewhere in the analysis, Anderson goes so far as to elaborate phenomena such as this into a general principle of social change: "Contrary to widely received beliefs among Marxists the characteristic "figure" of a crisis in a mode of production is not one in which vigorous (economic) forces of production burst triumphantly through retrograde (social) relations of production, and promptly establish a higher productivity and society on their ruins. On the contrary, the forces of production typically tend to *stall* and *recede* within the existent relations of production. In other words, the relations of production generally change *prior* to the forces of production in an epoch of transition, and not vice versa" (*Passages,* p. 204). There is, however, no explanation of why this "principle" should hold. At times, Anderson (*Lineages,* pp. 421–22) verges on mystical rhetoric in rejecting evolutionary chronology:

> Contrary to all historicist assumptions, time was as if at certain levels inverted between the first two [modes of production], to release the critical shift to the last. Contrary to all structuralist assumptions, there was no self-moving mechanism of displacement from the feudal mode of production to the capitalist mode of production, as contiguous and closed systems. The *concatenation* of the ancient and feudal modes of production was necessary to yield the capitalist mode of production in Europe—a relationship that was not merely one of diachronic sequence, but also at a certain stage of synchronic articulation. The classical past awoke again within the feudal present to assist the arrival of the capitalist future, both unimaginably more distant and strangely nearer to it. For the birth of capital also saw, as we know, the rebirth of antiquity. The Renaissance remains—despite every criticism and revision—the crux of European history as a whole: the double moment of an equally unexampled expansion of space, and recovery of time.

III

The decay of the ancient mode of production is followed eventually by the advent of the feudal mode of production. It is essential to determine the mechanism of this transition. Anderson relies upon a diffusion theory to account for the evolution of feudalism.[6] A diffusion theory is appropriate to describe the consequences of the interaction between two social formations only if it can be safely assumed that each of the social units is an autonomous whole. The assumption may be warranted in this particular case.

In his analysis, feudalism is seen to grow directly out of the interaction of the Roman imperial system with the social formations in its hinterland and beyond. Anderson refers to it as a catastrophic collision of two dissolving anterior modes of production. The first of these, the ancient mode, has been delineated above. The second mode Anderson terms Germanic, following Marx and Engels. The Germanic communities were characterized by exclusive communal property rights, a social organization based upon kinship groups, bands, or tribes, and hence no state to speak of. Peasants resided in settled communities and engaged either in arable production in allodial units or in pastoral agriculture. (It should be noted in passing that this description is sufficiently vague as to include within it Celtic social formations as well as Germanic ones;[7] however, this lack of specificity does not necessarily reduce the power of the analysis below.

The key point, long appreciated by medievalists (see Koebner 1965), is that Roman institutions intruded upon European territories with highly differential influence. In those parts of Europe influenced by Rome, three separate zones may be distinguished. Each zone had a characteristic mixture of elements from both the Roman and the Germanic modes of production. In southern Europe (Italy, Spain, southern France), the Rhone country, and the western Alps, Roman influence was strongest, since these areas had been fully incorporated into the Roman imperial system. In northwest Europe, the Roman legacy was weak (as in England prior to the Norman Conquest) or nonexistent (as in Scandinavia). Therefore, even during the height of Roman power, the social structure of these regions remained predominantly Germanic. Between these two areas lay a zone of "balanced synthesis" between the Roman and Germanic modes of production. This third zone, roughly coterminous with the heartland of the Carolingian Empire, ultimately produced the feudal mode of production in its classic form. Note the specificity of Anderson's description, which he ascribes to Marx's discussion of the work of Kovalevsky, published only in Russian: "Feudalism typically involves the juridical serfdom and military protection of the peasantry by a social class of nobles,

[6] For a discussion of diffusion theories and their limitations, see Hechter (1975*b*).

[7] An excellent description of the differences between Celtic and Germanic types of social organization is included in Homans (1941).

enjoying individual authority and property, and exercising an exclusive monopoly of law and private rights of justice within a political framework of fragmented sovereignty and subordinate fiscality, and an aristocratic ideology exalting rural life" (*Lineages*, p. 407). Just as in the treatment of the ancient mode of production, the superstructural level is given notable emphasis here, even down to a specification of the ideology of the dominant class.

The basis of the feudal economy was manorial. Within this heavily agrarian economy, substantial technical improvements occurred The use of the heavy iron plow, improved harnesses, the substitution of the horse for the ox in farm labor, and the three-field system of crop rotation all contributed to significant increases in productivity. Larger surpluses stimulated the development of local trade between manorial units. This trade, in turn, led to the establishment of centrally located market towns, which arose to minimize transport costs between trading producers (Nell 1967, p. 336). By the 12th century, market towns of this type began to be distributed evenly throughout the economic system.

But for Anderson (*Passages*, pp. 148–49), the single most important attribute of feudalism is to be found, not in the sphere of production—serfdom hardly plays the stellar role in his account—but in the sphere of politics. It is the "parcellization of sovereignty" which gives to feudalism its distinctive character:

> The peasants who occupied and tilled the land were not its owners Agrarian property was privately controlled by a class of feudal lords, who extracted a surplus from the peasants by politico-legal relations of compulsion This extra-economic coercion, taking the form of labour services, rents in kind or customary dues owed to the individual lord by the peasant, was exercised both on the manorial demesne attached directly to the person of the lord, and on the strip tenancies or virgates cultivated by the peasant Its necessary result was a juridical amalgamation of economic exploitation with political authority The peasant was subject to the jurisdiction of his lord At the same time, the property rights of the lord over his land were typically of degree only, he was invested in them by a superior noble (or nobles), to whom he would owe knight-service —provision of a military effective in time of war. His estates were, in other words, held as a fief The liege lord in his turn would often be the vassal of a feudal superior, and the chain of such dependent tenures linked to military service would extend upwards to the highest peak of the system—in most cases, a monarch—of whom all land could in the ultimate instance be in principle the eminent domain The consequence of such a system was that political sovereignty was never focused in a single center. The functions of the State were disintegrated in a vertical allocation downwards, at each level of which political and economic relations were, on the other hand, integrated This parcellization of sovereignty was constitutive of the whole feudal mode of production

Anderson claims that the lack of a unitary political authority had three significant consequences; in point of fact, only one of them is fundamental to his argument. First, parcellized sovereignty permitted communal

village lands and peasant allods to survive within a dominantly manorial economy. These stimulated agricultural productivity, though the analysis never makes clear precisely how this end was realized. Second, the ambiguous and limited authority of the king—who was not a supreme sovereign set above his subjects, but a suzerain of his vassals—created "an inbuilt contradiction within feudalism, between its own rigorous tendency to a decomposition of sovereignty and the absolute exigencies of a final centre of authority in which a practical recomposition could occur (*Passages,* p. 152). This lent to feudalism a certain kind of dynamic tension, in Anderson's eyes.

But clearly, the real cause of feudal dynamism lay in the fact that parcellized sovereignty allowed for the development of politically autonomous towns within the interstices of the feudal system. Unlike the cities of antiquity or those of imperial China, feudal cities were self-governing communes independent of the nobility and the Church. They were to develop into the spawning ground of a new and revolutionary class, the bourgeoisie. Feudalism was, as Marx had insisted, the first mode of production in history which caused the opposition between town and country. And, although Anderson does not make it clear, only those social formations which possessed both a strong aristocracy and a strong bourgeoisie would develop into the core states of the European world economy. This conclusion can be deduced from his argument by observing developments in the other zones of European feudalism.

First, consider the area of Roman-dominated synthesis, mostly confined to southern Europe. Upon the collapse of the system of latifundia production, many of the Roman urban communities had perished. But other cities survived in the south—many more than in the north (Jones 1966). Following the expansion of the European economy in the tenth century, these surviving Roman cities differed from the towns in the zone of balanced synthesis. They were the places of residence of a class of aristocrats gaining a *rentier* income from their countryside estates (Merrington 1975). Cities in this zone were more likely to be parasitic centers of consumption than the northern towns, which arose to fulfill central place functions in local (intermanorial) trade. Far from arising endogenously out of a relatively self-sufficient economy, the southern towns were dependent upon a Mediterranean-focused, long-distance trade They gained revenue by being centers of transshipment and of the production of manufactures for export to the international economy, limited as it was in this era. The southern German towns arose to fulfill similar functions. Towns in northern Italy and southern Germany were not strongly linked to their surrounding hinterlands; instead, they were nodal points in a network of long-distance trade. Consequently, in these territories the bourgeoisie and artisanry were relatively more numerous than in any other part of Europe. The urban classes, in this zone of deepest Roman influence, established a kind of political hegemony which could not be achieved by their comrades elsewhere. While a much more even balance between urban and rural classes had been struck in central west Europe, in the rest of the

continent—in those lands to the north of the current French border and to the east of the River Elbe, save for the towns of southern Germany—there was a contrary pattern of political hegemony. In this zone of weakest Roman influence, none of the indigenous modes of production—whether Germanic, Slavonic, or nomadic pastoralist—tended to produce substantial urban settlements. Here the landed classes dominated their respective polities. When long-distance trade penetrated eastern Europe, it was the nobility, not the indigenous urban bourgeoisie, which reaped most of the resulting profits.

The steppe lands of Pontic Europe were particularly bare of urban settlements. This was not so much the fault of the mode of production of its original inhabitants (as Anderson would have it) but was due principally to the fact that the area was a frontier for three expanding empires in the eastern part of the continent: the Russian, the Ottoman, and the Habsburg (MacNeill 1964). As such, it was the site of repeated invasion and conquest. Cities were impossible to defend in these circumstances. Though southeastern Europe had been extensively Romanized, the invasions of Slavic groups following the fall of Rome effectively broke this region's historical continuity, and Roman social institutions—which had survived in southwestern Europe—became extinct in much of the Balkans.

By now it should be apparent that parcellized sovereignty was not evenly distributed throughout Europe. This critical concept associated with feudalism is most relevant to the zone of balanced synthesis. Elsewhere, the principle of limited authority was undermined by the growing political hegemony of a single dominant class—either urban (as in much of southern Europe) or rural (as in much of eastern Europe). This does not by any means imply that the two latter zones developed effective central states before the former did. It is the paradox of European history that the absolutist state first developed precisely in those territories where the political authority of any one social class had been most limited during the Middle Ages. To me, this analysis of feudalism suggests an explanation for the skewed distribution of state power in Europe similar to that once advanced by Engels,[8] yet attacked by Anderson (*Lineages*, pp. 15–42).

Why were strong states most likely to emerge in those territories having both a numerous and well-established landowning nobility and a politically autonomous bourgeoisie? State formation must have grown out of the conflict between them. Each of these groups was jealous of the potential power of the other; each rested upon somewhat different productive imperatives—production for exchange on the part of the urban strata,

8 "Exceptional periods, however, occur when the warring classes are so nearly equal in forces that the state power, as apparent mediator, acquires for the moment a certain independence in relation to both This applies to the absolute monarchy of the 17th and 18th centuries, which balanced the nobility and the bourgeoisie against one another. . . ." (Engels 1972, p. 231).

production largely for use on the part of the rural strata. Each group rested upon distinctive cultural and ideological principles—ascriptive hierarchy in the case of the nobility, de jure equality in the case of the bourgeoisie. Finally, each saw its immediate economic interests as antagonistic —constrained markets in land and labor were desired by the aristocracy, whereas free markets in these factors were desired by the bourgeoisie.

The state, for its part, should be seen as an interested stratum *sui generis*, rather than as the captive of either class. The crown and its bureaucratic apparatus were always trying to maximize their revenues (most of which, toward the end of the feudal period, went to establish standing armies, thereby to subdue potential competitors, and to create ever larger bureaucracies, the better to tax subjects). In territories wherein either the nobility or the bourgeoisie could achieve virtual hegemony over civil society, strong states did not quickly emerge. This affinity of strong states with well-developed commercial and agrarian classes is nothing at this point but a correlation; it cannot explain the processes by which particular states actually achieved their strength. Here much detailed study is required, but some speculation may be warranted.

It appears that to the extent the state had the opportunity to play its potential rivals off against one another, its chances to achieve preeminence in the social formation were increased. This has been argued by Roland Mousnier (1954, p. 236): "Dividing the functions between the two classes, but offering the most important of them to the lesser class, that is the bourgeoisie, systematically raising it and setting it off against the nobility, the King brought the struggle of the classes to such an equilibrium that he could strengthen his own personal power and ensure in the government and in the state unity, order, and hierarchy." Of course, the potential rivals for power within a given social formation need not have been representatives solely of different classes. They might also have represented competing regional interests Prerevolutionary France, for example, was a society with three different economic sectors, oriented respectively toward the Atlantic, the Mediterranean, and the dorsal spine of Europe (Fox 1971; Wallerstein 1974*a*). Elites in these three regions, in some sense sharing membership in the same class, actually had competing economic and political interests at particular times.

Once the state achieved autonomous power, it did not cease being dependent upon the various classes and strata of civil society—for tax revenue is the lifeblood of all states—but it did cease its dependence upon *any one class*. The state's functional role in the early absolutist period was to mediate between the conflicting interests of civil society; this is largely how it initially gained power. Strong states were not perceived to be necessary by any class which had already achieved hegemony over civil society in the early modern period. Neither the landed aristocracy of Poland nor the urban burghers of southern Germany and northern Italy felt they had need of an autonomous state in the 16th and 17th centuries. Since all of their internal rivals had been subdued, they assumed that the social order was assured. But this shortsightedness on their part, this

desire to escape the cost of a large bureaucracy and the threat to their power which it might come to represent, was paid for dearly. For of course not all of the enemies of the hegemonic classes were located within the territorial domain of the state. Neighboring states were as likely as any internal competitors to engage in this struggle for hegemony.[9] The Polish nobility (*Szlachta*) discovered too late that their purposefully weak state structure could not prevent Polish lands from seizure at the hands of hostile powers from beyond its borders.

Indeed, the threat of invasion led occasionally to effective centralized governments in territories which otherwise lacked the structural preconditions for strong state formation. Sweden and Prussia, for example, had no significant urban concentrations, yet both established state apparatuses which, in the first instance, protected their lands from invasion.

How did states actually gain power at the expense of their internal competitors? The English case seems particularly apposite here: for about 100 years after the Reformation, the crown played a balancing act between capitalist and feudalist elements in civil society—supporting the feudal magnates by enacting the Poor Laws to keep the peasants on the land, *yet aiding the capitalist element in its policies concerning trade and manufacture.* Finally, in this regard, it cannot be accidental that periods of aggressive statism in England and France followed hard upon the heels of major civil wars in each society: the Wars of the Roses in the former, and the Fronde and the Wars of Religion in the latter. The state thus gained power initially by promising to put an end to internal violence, by offering a kind of social order in which agriculture, trade, and artisanal manufacturing could all flourish (Nell 1967, p. 336).

IV

The reason Anderson does not advocate this conception is that it gives too much credence to the position that the absolutist states are capitalist, or protocapitalist, social formations. Anderson insists this is a mistaken position for three reasons. First, the differences between the absolute monarchies and the (mostly) bourgeois democracies of industrialism are too great to allow them to be placed in the same category. Second, extraeconomic coercion persists in the absolutist era (though its geographical distribution is highly skewed); hence *by definition* the existence of the capitalist mode of production is precluded.[10] Third, since the dominant polit-

[9] Thus, S. E. Finer relates that in Germany and Italy "the 'conquest centre' failed to create any kind of system out of the peripheral sub-systems, partly because of their relative military resources, but primarily because they were supported by the resources of rival 'conquest centres'. The French-Swedish alliance in Germany against the Emperor in the Thirty Years' War tipped the scales decisively in favour of the peripheral sub-systems, allowing them to become independent" (1975, pp 95–96).

[10] But is this really a useful definition, let alone an accurate one? To be sure, under capitalism surplus is appropriated largely through contractual agreements between parties. This is not, however, strictly *economic* coercion because *the state* in the last

ical class in the absolutist states is the (feudal) nobility, it is absurd to consider these social formations as capitalist. Instead, absolutism in western Europe is described as "a redeployed and recharged apparatus of feudal domination designed to clamp the peasant masses back into their traditional social position—despite and against the gains they had won by widespread commutation of dues" (*Lineages*, p. 18).

Wallerstein, for his part, has attacked such reasoning by claiming that the capitalist mode of production must be specified at the level of the world economy. Superstructural differences between states participating in the world economy are not only likely to exist, they are in a fundamental sense *constitutive* of capitalism. For the mode of surplus appropriation in the capitalist world system "involves not only appropriation of the surplus-value by an owner from a laborer, but an appropriation of surplus of the whole world-economy by core areas. And this was as true in the stage of agricultural capitalism as it is in the stage of industrial capitalism" (Wallerstein 1974*b*, p. 401). And further, "the strength of the state machinery in core states is a function of the weakness of other state-machineries" (p. 403).

There is a plausible case to be made on either side of the argument. Note that two issues are really at stake. The first is the unit of analysis: To what social units does the concept of mode of production apply? The second, following from the first, is the importance of superstructural differences in the definition of the mode of production. The ontogenetic perspective tends to stress the peculiarities of the superstructure,[11] whereas the world-system approach tends to be more structurally determinist.

The choice between these competing perspectives must be made on empirical, not conceptual, grounds. And both Anderson and Wallerstein recognize that the critical phenomenon to be explained is the increasing structural divergence which developed between eastern and western Europe from the 17th century on. In solving this particular problem, the world-system persepective is clearly preferable to its ontogenetic alternative.

Within some limits both scholars agree about the facts to be explained.[12]

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analysis guarantees the sanctity of these contractual relations Indeed, capitalism requires that there be unprecedented strength at the political center of the social formation

[11] Hence, Anderson (*Passages*, pp 397–431) argues that Japan, although it had a feudal social organization comparable in some respects to classical western European feudalism, did not endogenously develop industrial capitalism because the classical legacy (quiritary law) was not available to it While this statement is incontrovertible, the causal status of this superstructural factor is by no means established. Indeed, it is probably possible to make a structural case, based upon a world system perspective, to account for the same facts

[12] This is not to deny the importance of the empirical disagreements between the two writers Anderson (*Lineages*, pp 196–97) suggests

> that the pull of the more advanced Western economy on the East has often been exaggerated in this epoch, as the sole or main force responsible for the manorial reaction there. In fact, while the corn trade undoubtedly intensified servile exploitation in Eastern Germany or Poland, it did not inaugurate it

These include the following: In western Europe, statism occurred earlier and to greater effect than in the east, where state formation was delayed by at least a century and in certain cases (in Poland, for example) did not occur at all. The economies of the core western states became increasingly diversified, while those of the east turned toward monoculture. Urbanization increased in the west and decreased in the east. Wage labor characterized the west, whereas the east saw the so-called Second Serfdom. The bourgeoisie of the west was strong and native; in the east it was weak and alien. Culturally, the population of the core western states became largely homogenous, whereas in the east pluralism was encouraged.

There is simply no way that an ontogenetic theory can account for this systematic pattern of structural divergence. Indeed, the diffusion theory which Anderson employs to such great effect in his discussion of the evolution of feudalism would predict a precisely opposite result from the undeniable increase in east-west interaction: that is, a structural convergence between east and west. Why wasn't there a synthesis between the eastern and western forms of absolutism comparable to that which apparently occurred between the Roman and Germanic modes of production? The answer may well be that, as Wallerstein has surmised, the social formations of western and eastern Europe had by this era become interdependent parts of a single system in which the positions of core and periphery can be distinguished.

Given the existence of such a system, in which core, peripheral, and semiperipheral territories compete with each other in zero-sum fashion for the surplus in the world economy, increasing structural differences between these positions is a likely outcome and perhaps even a necessary one. For this reason, the politically sovereign territory cannot usefully be seen as the proper social unit in the capitalist mode of production. For this reason as well, no diffusion model of social change can hold between social formations located within the capitalist world system—although such models may indeed be appropriate to previous historical periods.

> in either country, and played no role at all in the parallel development of Bohemia or Russia. In other words, if it is incorrect to ascribe central importance to the economic bonds of the export-import trade from East to West, this is because the feudal mode of production as such—by no means finally surpassed in Western Europe during the 16th and 17th centuries—could not create a unified international economic system, it was only the world market of industrial capitalism that accomplished this, radiating out from the advanced countries to mould and dominate the development of backward ones

For a similar assessment of the evidence see Brenner (1976, esp pp 47–60). Wallerstein, on the other hand, asserts, "What was happening in Europe from the 16th to the 18th centuries is that over a large geographical area going from Poland in the north-east westwards and southwards throughout Europe and including large parts of the Western Hemisphere as well, there grew up a world-economy with a single division of labor within which there was a world market, for which men produced largely agricultural products for sale and profit. I would think the simplest thing to do would be to call this agricultural capitalism" (1974*b*, p 399). Until more systematic research has been carried out, this debate must remain open.

Finally, by wedding Anderson's discussion of the development of feudalism to Wallerstein's analysis of the world system, a much more complete understanding of history is gained. For Wallerstein's structural analysis cannot satisfactorily account for the transition from one mode of production to another—it does not explain why this particular world economy evolved when it did. His discussion of the development of core, peripheral, and semiperipheral positions in the world system proceeds upon the gratuitous assumption that structural differences between the various European territories were both minimal and historically "accidental" (whatever the latter means) prior to the 17th century. Anderson's study successfully refutes this position by providing a convincing explanation for the evolution of these structural differences. His explanation accounts for why western Europe, and not eastern Europe, became the core area of the world economy.

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Review Symposium

The Modern World-System: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century. By Immanuel Wallerstein New York and London: Academic Press, 1974. Pp. xiv+410. $16.50/ £7.90.

WALLERSTEIN'S WORLD CAPITALIST SYSTEM: A THEORETICAL AND HISTORICAL CRITIQUE[1]

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Immanuel Wallerstein's *The Modern World-System* aims to achieve a clean conceptual break with theories of "modernization" and thus provide a new theoretical paradigm to guide our investigations of the emergence and development of capitalism, industrialism, and national states. This splendid undertaking could hardly be more appropriately timed and aimed. For quite some time, modernization approaches have been subjected to telling critical attacks (e.g., Gusfield 1967; Frank 1966; Bendix 1967, Tipps 1973; Smith 1973; Tilly 1975, chap. 9). They have been called to task for reifying the nation-state as the sole unit of analysis, for assuming that all countries can potentially follow a single path (or parallel and converging paths) of evolutionary development from "tradition" to "modernity," and, concomitantly, for disregarding the world-historical development of transnational structures that constrain and prompt national or local developments along diverse as well as parallel paths. Moreover, modernization theorists have been criticized for the method of explanation they frequently employ: ahistorical ideal types of "tradition" versus "modernity" are elaborated and then applied to national cases; if the evidence seems to fit, one assumes that a particular historical instance is adequately explained; if not, one looks for the "chance" factors that account for its deviation.

In the opening pages of *The Modern World-System,* and in a related essay (also published in 1974) called "The Rise and Future Demise of the World Capitalist System," Wallerstein unequivocally defines his approach in direct opposition to these features of modernization theory. Thus in his book he will concentrate on explaining the structure and functioning of

[1] I could not possibly have undertaken the challenging task of writing this review essay without the benefit of intellectual stimulation and thoughtful critical advice from many friends, students, and colleagues, including especially Michael Burawoy, Mounira Charrad, Daniel Chirot, Linda Frankel, Harriet Friedmann, Wally Goldfrank, Peter Gourevitch, Patrice Higonnet, George Homans, David Karen, Victor Perez-Diaz, Bill Skocpol, Dave Slaney, David Stark, Charles Stephen, Charles Tilly, Kay Trimberger, and Jonathan Zeitlin. However, none of these people is responsible for what I have finally decided to say here.

capitalism as a world economic system, viewing sovereign states as but "one kind of organizational structure among others within this single social system" (p. 7). Equally important, he intends to avoid the "intellectual dead-end of ahistorical model-building" (1974, p. 388) by grounding his theorizing in an analysis of the historically specific emergence and development of capitalism since the sixteenth century. He hopes thereby to demonstrate "that to be historically specific is not to fail to be analytically universal," that "the only road to nomothetic propositions is through the historically concrete" (1974, p. 391).

Given these very appealing and appropriate intentions of Wallerstein's theoretical program, not to mention the impressive scope of his reading in the works of historians, it is hardly surprising that *The Modern World-System* has met with an uncritically laudatory response from many sociologists. For example, Michael Hechter (1975) in his review of the book for *Contemporary Sociology* suggests that it provides a thoroughly plausible and internally consistent theoretical argument that needs only to be specified and operationalized to provide an adequate guide for fruitful research on development issues. But this assessment is too hasty and superficial. *The Modern World-System* is a theoretically ambitious work that deserves to be critically analyzed as such. And, as I shall attempt to show, Wallerstein's arguments are too misleading theoretically and historically to be accepted at face value. Because *The Modern World-System* does suffer from inadequacies of reasoning and evidence, there may be hypercritical reviews that will use the book's weaknesses as an excuse for dismissing out of hand any such world-historical or Marxist-oriented approach. With such an evaluation I have no sympathy. Like many other important pioneering works, Wallerstein's *Modern World-System* overreaches itself and falls short of its aims. It is therefore incumbent especially upon those of us who are sympathetic to its aims to subject this work to rigorous critical scrutiny. For the true contribution of *The Modern World-System* will lie, not in the proliferation of empirical research based uncritically upon it, but in the theoretical controversies and advances it can spark among its friends. In this spirit, let me begin the necessary process of critique in this review essay.

I

Despite his avowed desire to avoid "abstract model building," Wallerstein in fact deals with historical evidence primarily in terms of a preconceived model of the capitalist world economy. I shall, therefore, start by describing and discussing this model, before proceeding to consider its adequacy for explaining historical developments in early modern Europe.[2]

Wallerstein insists that any theory of social change must refer to a "social system"—that is, a "largely self-contained" entity whose develop-

[2] This essay does not pretend to present an adequate overview of *The Modern World-System*. A good sense of the scope and richness of the work is conveyed in the reviews by Hechter (1975), Lenzer (1974), and Thomas (1975).

mental dynamics are "largely internal" (p. 347). For self-containment to obtain, he reasons, the entity in question must be based upon a complete economic division of labor. Leaving aside small-scale, isolated subsistence societies, there have been, he says, only two kinds of large-scale social systems: (1) empires, in which a functional economic division of labor, occupationally not geographically based, is subsumed under an overarching, tribute-collecting imperial state, and (2) world economies, in which there are multiple political sovereignties, no one of which can subsume and control the entire economic system. A world economy should be, in Wallerstein's view, more able than a world empire to experience sustained economic development precisely because economic actors have more freedom to maneuver and to appropriate and reinvest surpluses.

Such a world economy—of which capitalism from the sixteenth century to the present has been (according to Wallerstein) the only long-lasting historical instance—is based upon a geographically differentiated division of labor, featuring three main zones—core, semiperiphery, and periphery—tied together by world market trade in bulk commodities that are necessities for everyday consumption. Each major zone of the world economy has an economic structure based upon its particular mixture of economic activities (e.g., industry plus differentiated agriculture in the core; monoculture in the periphery) and its characteristic form of "labor control" (e.g., skilled wage labor and tenantry in the core; sharecropping in the semiperiphery; and slavery or "coerced cash-crop labor" in the periphery). The different zones are differentially rewarded by the world economy, with surplus flowing disproportionately to the core areas. Moreover, the economic structure of each zone supports a given sort of dominant class oriented toward the world market, as well as states of a certain strength (strongest in the core and weakest in the periphery) that operate in the interests of that class. Finally, according to Wallerstein, the differential strength of the multiple states within the world capitalist economy is crucial for maintaining the system as a whole, for the strong states reinforce and increase the differential flow of surplus to the core zone. This happens because strong states can provide "extra-economic" assistance to allow their capitalist classes to manipulate and enforce terms of trade in their favor on the world market.

II

Let us reflect for a moment upon this model as a whole. Historically, one of the most striking things about capitalism has been its inherent dynamism. From a world-historical perspective, we need to understand how and why capitalism emerged, has developed, and might one day pass from the scene. Wallerstein clearly appreciates the importance of these issues—yet he does not offer very many insights about them, either in *MWS* or in his "Rise and Demise" article (where he sketches an overview of four stages of world capitalist development from the sixteenth century to the present).

For one thing, Wallerstein's theory does not put him in a good position to explain the transition from feudalism to capitalism in Europe. The most obvious difficulty is the lack of any theoretical conception of the dynamics of feudalism, which is neither a "world empire" nor a "world economy" in Wallerstein's terms. To explain what he holds to be the demise of feudalism around 1450, Wallerstein (chap. 1) employs, first, an amalgam of historians' arguments about reasons for the crisis of feudalism (1300–1450) and, then, a series of teleological arguments about how the crisis "had to be solved" if "Europe" or "the system" were to survive. The emergence of the capitalist world system is presented as the solution. Thus in this one instance where Wallerstein actually discusses a supposed transition from one mode of production to another, he uses the language of system survival, even though such language is quite incongruous.

As for how world capitalism develops once it is established, although Wallerstein does assert repeatedly that the system is dynamic, he provides us with no theoretical explanation of why developmental breakthroughs occur. In the "Rise and Demise" article (1974), the momentous consequences of the technological innovations achieved in the Industrial Revolution are much discussed, but not a word is said about the causes of the Industrial Revolution. The only definite dynamics of Wallerstein's world capitalist system are market processes: commercial growth, worldwide recessions, and the spread of trade in necessities to new regions of the globe. Apparently the final demise of the system will come after the market has spread to cover the entire globe and transform all workers into wage laborers. But even the all-important dynamic of global expansion itself depends upon the occurrence of technological innovations—themselves unexplained.

In sharp contrast to his awkwardness and sketchiness in explaining dynamics, Wallerstein is very forceful on the subject of the stability of the world capitalist system. In theory, as we have seen, once the system is established, everything reinforces everything else. And Wallerstein consistently employs not only system-maintenance arguments but also direct analogies between the structure of the world capitalist system and the typical structure of political empires (e.g., pp. 349–50) to convey a sense of the massive stability of the whole. For he believes that his model points to the essential structures of world capitalism—to patterns of division of labor and of relationships among states in different economic positions that have endured since the sixteenth century even though the system as a whole has expanded geographically and particular countries have changed positions within the system.

III

Taking our cue from his emphases, then, let us take a close critical look at the ideas about determinants of socioeconomic and political structures that are built into Wallerstein's model of the world capitalist system. We can most readily pinpoint the problematic points, I suggest, if we see that the model is based on a two-step reduction: first, a reduction of socio-

economic structure to determination by world market opportunities and technological production possibilities; and second, a reduction of state structures and policies to determination by dominant class interests.

The ways in which Wallerstein tries to make sense of the differences of economic structure among his three major zones of core, semiperiphery, and periphery lead him to make the first reduction. The crux of the differences is the "mode of labor control" "adopted" in each zone by the dominant classes oriented to the world market. In his theoretical passages addressed to this issue (see esp. chap 2, pp. 87–116), Wallerstein repeatedly implies that the dominant classes choose freely among alternative strategies of labor control by assessing rationally the best means for maximizing profits, given the geographical, demographic, technological, and labor-skill conditions in which they find themselves, and given the profitable possibilities they face for selling particular kinds of products on the world market. Now the curious thing here is that, despite the fact that Wallerstein seems to be placing a great deal of stress on the class structures of the major zones of world capitalism, actually (as far as I can see) he is explaining the fundamental economic dynamics of the system in terms of exactly the variables usually stressed by liberal economists, while ignoring the basic Marxist insight that the social relations of production and surplus appropriation are the sociological key to the functioning and development of any economic system. For this Marxist idea demands that one pay attention to institutionalized *relationships* between producing and surplus-appropriating classes and allow for the ever-present potential of collective resistance from below. Instead, Wallerstein treats "labor control" primarily as a market-optimizing strategy of the dominant class alone.

One major theoretical effect of his reliance on liberal economics is a nonexploitative picture of the process of income distribution within the world system. To be sure, he argues that the forces of the marketplace tend to maintain established differences of "occupational" structure among regions (p. 350). But notice the reason offered: "a capitalist world-economy essentially rewards accumulated capital, including human capital, at a higher rate than 'raw' labor power . . ." (p. 350). Would a liberal economist say anything different, since all that is being argued here is that regions with the scarcer factors of production are differentially rewarded by the market?

Yet, of course, Wallerstein does argue theoretically that the structure and functioning of the world capitalist economy are inherently exploitative. He does so by assigning the international hierarchy of dominating and dominated states (especially core vs. periphery) a crucial mediating role in exacerbating and sustaining overall inequalities in the system as a whole. Thus he writes, "Once we get a difference in the strength of the state-machineries, we get the operation of 'unequal exchange' which is enforced by strong states on weak ones, by core states on peripheral areas. Thus capitalism involves not only appropriation of surplus-value by an owner from a laborer, but an appropriation of surplus of the whole world-economy by core areas" (1974, p. 401).

But, then, how are degrees of state strength and kinds of state economic policies to be explained? Here we arrive at the second reduction built into Wallerstein's model. For in his theory, differences of state strength and policies among states located in different major zones of the world system are explained as the result of differences in regional rates of surplus appropriation and, above all, as the expressions of the different world market interests of the dominant classes within the national political arenas that happen to be located in each major zone (chap. 3, passim). Thus the core area ends up with strong states primarily because there are more plentiful surpluses to tax and because the dominant capitalist classes want state protection for industry and their control of international trade; on the other hand, the periphery ends up with weak or nonexistent states because it reaps less from world trade and because its dominant capitalist classes are interested in profiting from direct dealings with merchants from the core areas. In short, to explain differences in state strength, Wallerstein relies upon arguments about economic conditions and world market interests, largely ignoring other potentially important variables such as historically preexisting institutional patterns, threats of rebellion from below, and geopolitical pressures and constraints.

Given that the economic structure and functioning of the world system have (logically speaking) already been explained in market-technological rather than class terms, Wallerstein *must* make this second reduction, of politics to world market-oriented class interest, in order to be able to assert that the system will be exploitative, and stably so over the long run. For as he points out, if states were equally strong (or potentially equally strong across the major regions), "they would be in the position of blocking the effective operation of transnational economic entities whose locus [*sic*] were in another state. It would then follow that the world division of labor would be impeded, the world-economy decline, and eventually the world-system fall apart" (p. 355). Without a hierarchy of dominating and dominated states corresponding to the existing pattern of economic differentiation, there is no worldwide "unequal exchange" in this theory. Ironically, then, Wallerstein has managed to create a model that simultaneously gives a decisive role to international political domination (curiously enough for a theory that set out to deemphasize the nation-state!) and deprives politics of any independent efficacy, reducing it to the vulgar expression of market-class interests.

Well, so what? Do these theoretical peculiarities matter? Certainly some quite implausible assumptions have to be made to make the model internally consistent. Since everything is directly or indirectly an expression of capitalist class interests (under given technical conditions), we are forced to assume that these classes always get what they want, reshaping institutions and their relations to producing classes to suit their current world market opportunities. At the same time, we must assume that, although all of the variously situated dominant capitalist classes want and are able to maximize their world market trading advantages, nevertheless *only* the

core-area capitalists want, need, and get the extra-economic assistance of strong states, while peripheral capitalists do not.

Still, the peculiarities and implausibilities would not matter very much if the model itself were genuinely useful for analyzing and explaining actual historical developments. But I believe that each of the two reductions in Wallerstein's model deprives him of crucial explanatory resources for understanding the patterns of history. Let me argue my case by examining in turn two major early modern European developments that Wallerstein himself stresses in *The Modern World-System:* (1) the resolution of the "crises of feudalism" into opposite socioeconomic structures in Eastern versus Western Europe; and (2) the emergence of monarchical absolutisms. Afterwards I shall draw some tentative conclusions about the overall validity of Wallerstein's model and about a possible alternative approach.

IV

One of the most striking developments in Europe during the "long sixteenth century" (1450–1640) was the divergence of economic patterns between northwestern Europe and Eastern Europe (including, e.g., Poland, Hungary, Livonia, and Germany east of the Elbe River). While in the West serfdom was virtually gone by 1600, and thereafter the commercialization of the social relations of agrarian production and the growth of industries were important trends, in the East the peasants had by 1600 become tied to the land so that labor and dues could be forcibly extracted from them by the landlords, and this so-called second serfdom was accompanied by the decline of towns and indigenous industries under bourgeois control. Moreover during the same general period East and West became more intensively linked through the Baltic trade, in which primary bulk goods, including especially grain, were exported from the East, which in turn imported manufactures, primarily from England and the Netherlands.

Clearly this pattern corresponds very nicely to Wallerstein's model of relations between core and periphery in the emergent capitalist world economy. This in itself is not really surprising, though, since Wallerstein's model, as he fully acknowledges, was originally inspired in part by the work of Marian Małowist, a historian who stresses the importance of the Baltic trade as a contributing cause of the Eastern versus Western divergence. Yet what was for the historian one contributing cause becomes for Wallerstein, given the dictates of his world capitalist system model, *the* theoretically significant explanation. Thus he argues, "The reason why these opposite reactions . . . occurred was because . . . the two areas became complementary parts of a more complex single system, the European world-economy, in which eastern Europe played the role of raw-materials producer for the industrializing west . . ." (p. 95). "The crucial considerations in the form of labor control adopted in eastern Europe were the opportunity of large profit if production were increased (because of the existence of a world market), plus the combination of a relative shortage of labor and a large amount of unused land" (p. 99).

To be really convincing, Wallerstein's explanation should meet two conditions. First, it should be validated (or at any rate not invalidated) by the timing, or sequence, of events; that is, if world trade opportunities really were the decisive cause of the "second serfdom," their availability should precede, or at least fully coincide with, the trends toward enserfment. But actually the process of enserfment was under way in virtually all areas by 1400 (Blum 1957, p. 820), and by "the end of the fifteenth century [i.e., 1500] . . ., from the Elbe to the Volga, most of the peasantry were well on the way to becoming serfs" (Blum 1957, p. 821; see also Carsten 1954, chap. 8; Slicher Van Bath 1963, pp. 156–57), whereas Eastern grain exports to the West began expanding significantly around 1500 and achieved their most sudden and sizable growth only between 1550 and 1600 (Malowist 1958, pp 27–29), *after* the foundations of the coerced labor system were fully established.

Second, and more important, Wallerstein's emphasis on trade opportunities (as well as technical production possibilities) ought to be able to stand critical scrutiny in the light of comparative historical evidence. However, an important article by Robert Brenner (1976) entitled "Agrarian Class Structure and Economic Development in Pre-industrial Europe" strongly suggests that Wallerstein is misguided. Brenner shows that markets cannot solely or primarily explain social-structural transformations or economic developments because, depending upon the preexisting institutional patterns of class relations, different classes may be in the best position to take advantage of available trade opportunities and thereby have their particular positions strengthened. Thus Brenner points out (1976, p. 53) that in parts of northwestern Germany in the 16th century peasants (rather than enserfing lords) took advantage of the new export opportunities—"and they appear to have done so after a prolonged period of anti-landlord resistance." As for the Eastern lords, Brenner concludes (1976, p. 53), "No doubt, in this instance, the income from grain produced by serf-based agriculture and sold by export . . . enhanced the class power of the Eastern lords, helping them to sustain their seigneurial offensive. But the control of grain production (and thus the grain trade) secured through their successful enserfment of the peasantry was by no means assured by the mere fact of the emergence of the grain markets themselves." Rather, as even Malowist (Wallerstein's historical source) says (1958, p. 38), "trade developed in a form determined by locally prevalent social and economic circumstances and affected these in turn."

Brenner's carefully crafted comparative historical investigations suggest that to explain the divergences of socioeconomic developments in East versus West we must attend especially[3] to the "*historically specific* pat-

[3] Differences of political history and of the relative strength of towns have also to be taken into consideration in order to explain the socioeconomic divergences of East and West Wallerstein mentions these matters briefly (pp 97–98); they are discussed at much greater length in Blum (1957) But Brenner (1976, pp. 54–56) raises some important caveats about the influence of towns on the course of agrarian class struggles

terns of development of the contending agrarian classes and their relative strength in the different European societies: their relative levels of internal solidarity, their self-consciousness and organization, and their general political resources . . ." (1976, p. 52). Thus Brenner makes a case that Eastern peasants were more easily and thoroughly dominated by their landlords because, for various specific historical reasons, they enjoyed much less village community solidarity and local political autonomy than did Western European peasants. The Eastern peasants found it more difficult, especially over the long run, to resist the lords collectively. Consequently, when the Eastern lords attempted to impose coercive controls, initially under conditions of economic crisis and labor scarcity, they succeeded, whereas lords in the West had failed in the same attempt under similar conditions during the 1300s (see Hilton 1969).

Moreover, Brenner goes on to apply his explanatory approach to another issue relevant for Wallerstein's theory: the problem of why English feudal agriculture was transformed in early modern times into an agriculture based primarily on contract rents and wage labor, while French, as well as Eastern European, agriculture was not so transformed. The detailed argument (which I shall not reproduce here) suggests that Wallerstein's theoretical neglect of the independent significance of institutionalized patterns of class relationships deprives him of an important tool for actually explaining, rather than merely asserting, both the "rise of the gentry" and the occurrence of capitalist economic-technological breakthroughs in English agriculture. For, as Brenner argues (1976, p 37), "[E]conomic development can only be fully understood as the outcome of the emergence of new class relations more favourable to new organizations of production, technical innovations, and increasing levels of productive investment. These new class relations were themselves the result of previous, relatively autonomous processes of class conflict."

V

If Wallerstein's world-market theory prevents him from adequately explaining patterns of economic development in early modern Europe, it leaves him even less able to make sense of the patterns of state development. This was, of course, the era of the initial emergence of absolute monarchies—kingly governments that tried, with varying degrees of success, to impose protobureaucratic administrative controls and coercive monopolies over large populations and territories. Wallerstein recounts the phenomena of absolutism (chap. 3) and tries to subsume them within his theory by invoking the category of the "strong state." According to the theory, let us recall, strong states necessarily grow up in the core zone of the world capitalist economy. Thus Wallerstein asserts, "In the sixteenth century, some monarchs achieved great strength. . . . Others failed. This

His observations suggest that noting the relative strength of towns is no substitute for direct analysis of agrarian class relations.

is closely related . . . to the role of the area in the division of labor within the world-economy. The different roles led to different class structures which led to different politics" (p. 157). "In the core states there evolved relatively strong State systems, with an absolute monarch and a patrimonial State bureaucracy working primarily for this monarch. The venality of office and the development of standing armies based on mercenaries were the critical elements in the establishment of such a bureaucracy" (1972, p. 96). However, Wallerstein's attempt to equate the strong core state and absolute monarchy does not work. The historical evidence simply does not fit the overall pattern implied by the theory, for there were more and stronger absolutisms outside the core than in it.

Economically speaking, both the Netherlands and England were, according to Wallerstein's analysis, core countries. Were they also strong states? The "strong state" is defined theoretically by Wallerstein (p. 355) as strong "vis-à-vis other states within the world-economy including other core states, and strong vis-à-vis local political units within the boundaries of the state . . . also . . . strong vis-à-vis any particular social group within the state." Since the Dutch government was simply a federation of merchant oligarchies, Wallerstein does not even try to convince us that the Netherlands really was a strong state; instead he stresses the economic interdependence of England and the Netherlands and the transitional functions of Dutch economic primacy for the emerging world capitalist system (chap. 4, pp. 199 ff.). But he clearly wants us to believe that the English Tudor state was a strong core state (pp. 231–33)—even though (as he himself admits, pp. 234–35) the English monarchs had no large standing armies and no bureaucratic administration that penetrated the localities. In fact, the English monarchs could rule only through cooperation with locally powerful notables, the county-Parliamentary gentry and the London merchant oligarchy.

What about the true absolute monarchies of Europe, such as the Spanish, the French, and the Swedish? Wallerstein stresses the bureaucratic weight and military aggressiveness of the Spanish state whenever he is trying to account for European domination of the New World and when (chap. 4) he discusses the Hapsburg attempt at empire building within Europe Then, suddenly, Spain drops out of the picture, even though her monarchy remained thoroughly absolutist and, arguably, just as internationally powerful as the English government throughout the entire historical period under consideration.[4] (Perhaps the English state was more effective in

[4] Apparently Wallerstein believes that he need not treat Spain and England as comparable in the same analytic terms Instead he builds a series of contrasts between Spain as a would-be empire and England as a would-be national state These arguments are fascinating and ring true in many ways. But they suffer from two difficulties First, they are usually teleological (and thus credible only if we grant that political rulers were extraordinarily farsighted about the emergence of a world economy, which the Spanish supposedly were trying to subsume and control, while the English supposedly were trying to become a core state). Second, their theoretical status is unclear because Wallerstein has given us no categories for analyzing the politics of

promoting certain protomercantilist policies, but if this alone were used as the index of "state strength," the entire argument would become circular —and, of course, Wallerstein's initial focus on "absolutism" would be belied.) As for France, the theoretically induced dilemma that Wallerstein faces is how to explain why this country, situated only partly in the core zone, partly in the semiperiphery, actually developed a much stronger state than did either England or the Netherlands. To cope with this dilemma, an alternative ad hoc (and, of course, teleological) explanation of state strength is introduced: France "had to" develop a centralized, bureaucratic monarchy in order to hold together her differently oriented capitalist classes (chap. 5, pp. 263–69, 283 ff.). Similarly, when another, even more blatantly deviant case comes up—that of Sweden, with probably the most powerful and dynamic absolutism of the era (see Anderson 1974, part 1, chap. 7)—Wallerstein introduces still another ad hoc explanation:

> The position of Sweden is worth brief attention, as the evolution of Sweden's state machinery approached the model of western Europe rather than that of the periphery, although it was economically very underdeveloped at this time It was strong, not because its commerce and industry was [*sic*] strong ; it was paradoxically rather that its agriculture was weak, and its aristocrats wished to take hold of the profits of other lands for want of being able to create them on their own As a peripheral state with a weak bourgeoisie, [Sweden] was an arena in which the political power of the aristocracy grew with the economic expansion of the sixteenth century. But the growth of wheat was hindered by the climatic downturn of the time which affected negatively in particular the Scandinavian countries The nobility hence needed conquest and for that they needed a strong, not a weak, state Once they had the strong state, they would be able in the seventeenth and eighteenth centuries to use mercantilism as a lever of industrial advance, and hence be spared the fate of Poland. [Pp 312–13]

But with this final explanatory maneuver, Wallerstein thoroughly contradicts his original assertion that the strongest absolutisms should emerge in the core and certainly not in the periphery. For Sweden demonstrates (as does Prussia, after 1650) that a very strong state can be built on a peripheral agrarian base, and that, once built, it can reshape the economic future of the area in question.

Clearly, neither the differential appearance of absolutist states in early modern Europe nor their effects upon economic development are adequately accounted for by Wallerstein's world capitalist system theory. Better (though not unambiguous or flawless) treatments of patterns of state development are to be found in Anderson (1974) and Tilly (1975). These works suggest that, although no simple or monocausal explanation of state building is possible, two main sets of variables can go a long way toward accounting for the variations. First, internal class structures were

European feudalism; thus, we do not know (except through ad hoc comments) the basis for the "imperial" aspirations of European aristocracies

important, not because economically dominant classes got automatically what they wanted, but because different patterns of class relationships and alliances—including relationships and alliances involving agrarian feudal classes—created different possibilities for monarchs to extract resources and encouraged them to use available resources in different ways. Second, transnational structures were important, too—including the networks of trade and economic interdependence to which Wallerstein points. But not only these: for an equally if not more important transnational structure was that constituted by the system of politico-military interactions among emerging European states. This "European states system" set up pressures, constraints, and opportunities, varying according to the specific geopolitical situation of each country, which helped determine the kinds, strengths, and policies (including economically relevant policies) of the states that developed (or did not develop) in various times and places. Here is a kind of "world system" that Wallerstein seems inclined to emphasize when he contrasts feudal Europe with China in chapter 1; moreover, it resonates with his theoretical stress on "multiple sovereignty" as a defining feature of capitalism. But, unfortunately, the independent reality and effects of a system of militarily competing states cannot be comprehended by a theory that reduces politics to the expression of market situation and class interests; so Wallerstein neglects this explanatory resource as well.

In early modern Europe, incessant military competition among monarchies was an important spur to, and arbiter of, strong state building, for the main use of enlarged royal tax or loan revenues was the building up of standing armies and their deployment in wars. Not surprisingly, those monarchies that found it both necessary and possible to extract the resources (by various means) to build the largest land armies were also the ones that developed the strongest and most bureaucratic administrative machineries (needed to tax the peasants and/or control commerce and/or absorb upper-class subjects). But these were not the countries that found themselves during this period at the center of the nascent capitalist commercial economy. The Netherlands, as Wallerstein himself points out, was a small country whose survival depended upon military balances among her powerful neighbors. And England could remain somewhat aloof from the continental military system (given the nature of the military and naval technology of the times) because of her island situation. Because of their prior political histories and relatively sheltered geopolitical circumstances, England and the Netherlands happened during this period to have governments uniquely responsive to commercial-capitalist interests. These were not bureaucratic governments (even by the standards of the time) and for that very reason they were not so strong (esp. the monarchy over against the dominant classes) as to be able to stifle commercial development or protect the lower classes (as the French monarchy did its peasantry) against encroachments upon their position or very existence by capitalist landlords or bourgeoisies.

Indeed, it was probably one necessary condition (as was England's increasing centrality in world trade) for continuing capitalist development

in early modern Europe that England's would-be absolutisms did not, in the final analysis, consolidate themselves. Because they did not, and because England's geopolitical situation allowed her to get along without a military absolutism, agrarian commercialization—which must itself be explained by reference to developments over time of class structure and conflict (see Brenner 1976)—could proceed unhindered, and eventually facilitate the Industrial Revolution Then, once capitalist relations of production and accumulation were firmly established in England, the dynamics of the European states system ensured that capitalist relations would spread both across Europe and over the entire globe through state initiatives by competing powers and through military conquests, as well as through market expansion.

Interestingly enough, in his detailed historical discussion of England, Wallerstein himself makes points about her geopolitical situation and state stucture similar to the ones I have made here. Indeed, the rich historical chapters of *The Modern World-System* (e.g , chaps. 4 and 5) provide many pointers for someone interested in developing new hypotheses about relationships among various dimensions of state strength and processes of capitalist development. The implicit hypotheses do not, however, square very well with Wallerstein's basic model of the world capitalist system For, if the strongest states are not always in the core and if, in fact, equally strong or stronger states can grow up in the periphery (not to mention the semiperiphery), then according to Wallerstein's own logic (pp. 354–55) the economic division of labor cannot be presumed likely to hold together over time as a "system" and the differential flow of surpluses to the core is likely to be disrupted. Empirically speaking, these disruptive possibilities seem especially likely in later stages of world capitalist development, when strong, noncore states, perhaps created through revolutions from above or below, may be able to initiate rapid industrialization or other programs of economic development. Perhaps we still sense that Wallerstein's vision of an enduring, exploitative division of labor is correct, but in that case the theoretical reasons why it is correct must be found elsewhere than in the market economics and the economic-reductionist political sociology of Wallerstein's own model of the world capitalist system.

Without pretending to offer a fully worked out alternative paradigm, I suggest that, instead of exclusively pursuing Wallerstein's world system approach, we should investigate the world-historical emergence and development of capitalism in terms of hypotheses about variations in both (1) institutionalized class relations of production and exchange, and (2) patterns of state structures and interstate relationships, without simply reducing the latter to the former. To be sure, markets and patterns of trade are bound to be part of the picture, but it seems unlikely that they can be understood in their origin, functioning, or effects except with reference to changes in class and political structures. The alternative picture of world capitalism that is likely to emerge from historical analyses pursued along these lines will probably pertain to intersecting structures (e.g., class structures, trade networks, state structures, and geopolitical systems) in-

volving varying and autonomous logics and different, though overlapping, historical times, rather than a single, all-encompassing system that comes into being in one stage and then remains constant in its essential patterns until capitalism as a whole meets its demise. But this is only meant to be suggestive, not definitive of a true alternative to Wallerstein. Others may prefer to retain his idea of a worldwide economic division of labor and seek to explicate it theoretically and ground it historically in new ways.

VI

Finally, aside from this substantive critique of Wallerstein's approach, two methodological criticisms need to be made. The first has to do with the way Wallerstein handles historical evidence in relation to his theory-building enterprise. In many of the arguments cited in this essay, we have witnessed the major method of argumentation to which Wallerstein resorts: the teleological assertion. Repeatedly he argues that things at a certain time and place had to be a certain way in order to bring about later states or developments that accord (or seem to accord) with what his system model of the world capitalist economy requires or predicts. If the actual causal patterns suggested by historical accounts or comparative-historical analyses happen to correspond with the a posteriori reasoning, Wallerstein considers them to be adequately explained in terms of his model, which is, in turn, held to be supported historically. But if obvious pieces of historical evidence or typically asserted causal patterns do not fit, either they are not mentioned, or (more frequently) they are discussed, perhaps at length, only to be explained in ad hoc ways and/or treated as "accidental" in relation to the supposedly more fundamental connections emphasized by the world-system theory.[5] Frankly, I find this aspect of Wallerstein's approach very disturbing because it has the effect of creating an impenetrable abyss between historical findings and social science theorizing. For, through his a posteriori style of argument, deviant historical cases do not force one to modify or replace one's theory, while even a very inappropriate model can be illustrated historically without being put to the rigorous test of making real sense of actual patterns and causal processes in history. This has been exactly the methodological shortcoming of modernization theories, and it needs badly to be overcome in any new paradigm for development studies!

Which brings me to my second and final methodological point. At the

[5] Why does Wallerstein resort to this a posteriori style of argument? In his "Introduction" he argues that astronomers use this mode of argument to explain the evolution of the universe—a unique system supposedly like the world capitalist system (pp 7–8). But, as Friedmann (1976) points out in a brilliant critique of Wallerstein, astronomers do not have historical evidence (not much, anyway) to test their hypotheses, whereas social scientists do. Yet I suggest that the very content of Wallerstein's theory makes it awkward for him to use historical evidence effectively For historians stress chronologically ordered causal processes, while Wallerstein's world market approach prompts him to stress synchronic interdependencies (see 1974, p. 403) and anticipatory acts on the part of profit-maximizing capitalists and "entrepreneur-like" (p. 60) nation-states

beginning of this review essay I pointed out that Wallerstein hoped to overcome the worst faults of modernization theories by breaking with their overemphasis on national states and their tendency toward ahistorical model building. Ironically, though, he himself ends up reproducing the old difficulties in new ways. Thus strong states and inter*national* political domination assume crucial roles in his theory—though, just like the developmentalists, he reduces politics to economic conditions and to the expression of the will of the dominant groups within each national arena! Moreover, as we have just seen, Wallerstein creates an opposition between a formalistic theoretical model of universal reference, on the one hand, and the particularities and "accidents" of history, on the other hand—an opposition that uncannily resembles the relationship between theory and history in the ideal type method of the modernization approach.

How could these things happen, given Wallerstein's original intentions? The answer, I suggest, is the "mirror image" trap that plagues any attempt to create a new paradigm through direct, polemic opposition to an old one. Social science may, as is often said, grow through polemics. But it can also stagnate through them, if innovators uncritically carry over outmoded theoretical categories (e.g., "system") and if they define new ones mainly by searching for the seemingly direct opposite of the old ones (e.g., "world system" vs. "national system"). For what seems like a direct opposite may rest on similar assumptions, or may lead one (through the attempt to work with an artificial, too extreme opposition) around full circle to the thing originally opposed. The better way to proceed is to ask what new units of analysis—probably not only one, but several, perhaps changing with historical points of reference—can allow one to cut into the evidence in new ways in order to investigate exactly the problems or relationships that the older approaches have neglected.

This review essay has obviously been a very critical one. In it I have grappled with a monumental and difficult book, trying to pinpoint and critically examine the theoretical essentials of its argument. No one should suppose, however, that I am suggesting that we dismiss or ignore Wallerstein's on-going study of the world capitalist system (for this is just the first of four projected volumes). On the contrary, I can think of no intellectual project in the social sciences that is of greater interest and importance. Even if Wallerstein has so far given imperfect answers about the historical development of capitalism, still he has had the unparalleled boldness of vision to raise all the important issues. Even the shortcomings of his effort, therefore, can be far more fruitful for the social sciences than many minute successes by others who attempt much less. No book could have been more deserving of the Sorokin Award than *The Modern World-System*—and no book is more worthy of continued attention and debate.

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A SOCIOLOGICAL PERSPECTIVE ON WALLERSTEIN

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Immanuel Wallerstein's book has been hailed by some as a tour de force because of its scope and his aspirations. As such, it deserves critical reviews in addition to those which heralded its publication.

Wallerstein's synthetic monograph, *The Modern World-System,* requires scholarly assessment by economic historians, that is, by those economic historians sympathetic to comparative history that emphasizes social structure. No doubt, in the course of academic events, this study will be judged by such specialists. But Wallerstein also offers his endeavor as a contribution to macrosociology and to comparative societal analysis. Sociol-

ogists must be concerned with it in the light of the traditions and present state of comparative macrosociology.

I read him in this perspective. My standpoint is that of a teacher who seeks to make the developing core of macrosociology available to apprentice sociologists in research training and to successive generations of college students interested in a liberal education. If there is a core in the sociological discipline, it is in the diverse and variegated elements in the study of macrosociology—from de Tocqueville's *The Old Regime and the French Revolution* to Drake and Cayton's *Black Metropolis*. How does Wallerstein's work relate to this intellectual core?

It is an enormously self-conscious undertaking presented in the grand manner, as manifested by the title, *The Modern World-System*. We have the first volume of a multivolume investigation of the political economy and political sociology of the emergence of the modern world. This installment encompasses "Capitalist Agriculture and the Origins of the European World Economy in the Sixteenth Century," which Wallerstein expands to include the years 1450–1640.

This is a study of societal transformation. Wallerstein believes that he is addressing the classic question: "Why did the West—Western Europe—develop first economically and first experience sustained economic growth?" The frame of reference is the contemporary version of economic determinism Economic considerations, including population, technology, and resources, have central importance through their effects on social stratification. The pattern of stratification is viewed, to use Lloyd Fallers's phrase, in "stratigraphic" terms, that is, as nationwide strata rooted in occupational roles and thus forming a small number of discrete classes Political relations are the direct manifestations of these socioeconomic relations. Into this frame of reference, Wallerstein infuses the contemporary concern with ecological and, by implication, geopolitical dimensions The economic variables operate in spatial patterns of gradients that tend either to concentrate or to attenuate the patterns of social stratification and of sociopolitical exploitation. Wallerstein's effort to include an ecological dimension in his notion of social class stratification is one of the intriguing aspects of his approach.

According to Wallerstein, one cannot analyze separate nation-states in an effort to explain economic expansion and the resulting patterns of exploitation. Rather, it is necessary to analyze change in terms of the relations among nations, that is, the "social system" that is created by the linkages among nations. One deals with the social system only when the hierarchy of nations is taken into account. The structure of his argument, designed to organize the mass of historical documentation, rests on typologies, particularly those that distinguish between empire and world economy, rather than on systemic or propositional analysis.

Therefore he asserts that the nation-state is not the appropriate unit of analysis. (I believe, incidentally, that, given his logic, he means the nation-state is not the appropriate object of analysis.) Instead, he believes it necessary to think of world history as the history of the transformation of

political empires into a world economy. A political empire is not capable of sustained economic growth; a world economy is. "World economy" is an alternative designation for capitalist economy and the required state apparatus. The world economy obviously extends beyond the political boundaries of particular nation-states. The central issue is the conditions under which political empires are transformed into a world economy, with the resulting sociopolitical hegemony.

Wallerstein's strategy is to compare Western Europe with China, and Western Europe with Eastern Europe, especially Poland and to a lesser extent the Hapsburg Empire. The political empires of China and of Eastern Europe could have developed before Western Europe or at least simultaneously, Wallerstein asserts. In particular, he argues, without convincing me, that the social structures of these parts of the world were similar to those of Western Europe. Of course, he is using "social structure" in his own very specialized way without referring to the normative structures of these areas, a usage which will separate him from a vast central segment of sociological inquiry. Even in his own terms, the similarity will certainly appear overstated, although the potential for economic development in China and Eastern Europe was at that time as great as that in the West. In any event, the comparison with China is too limited to figure centrally in his argument; his case rests on a comparison of Western with Eastern Europe. (He is also concerned with incorporating the case of the Italian city-states into his scheme of analysis.)

For Wallerstein, the economic relations which produced the world economy in Western Europe reflect both internally in the system of class relations within the Western nation-states and externally in their ecological position in the world trade system. In short, sociopolitical structures reflect an internal stratification hierarchy and an international hierarchy of territorial groups. Exploitation results from both internal and external hierarchies. Wallerstein speaks of the core nations, the periphery, and the semiperiphery. Specifically, the transformation of political empires into a capitalist world economy involved internal economic changes in the nation-state followed by the changes in the international system of trade that occurred in the 16th century. The result is that the world economy produces, and rests on, a center, a group of core nations with strong governments dominated by the bourgeoisie, strong cities, and wage labor; the periphery is dominated by an aristocratic structure, serflike labor, and weak cities. This transformation took place in Western Europe, but not in Eastern Europe, and produced economic arrangements that resulted in the accumulation of surplus capital, the cornerstone of a Marxian kind of analysis.

Internally, he points to the commercialization of agriculture in Western Europe and to the end of serfdom. Externally, he attributes economic expansion to the patterns of the world economy—namely, the system of colonial trade and colonial settlement, in which Spain and Portugal took the initial lead. The system generated a flow of economic surplus to the European heartland and created external peripheries to be exploited by

Western Europe. For Wallerstein, this external system of stratification and economic exploitation was crucial to the emergence of the Western-dominated world economy. Both the commercialization of agriculture and, apparently still more important, the revenues of colonialism created the capital with which to create the self-sustained economic growth of Western capitalism.

The picture of the development of capitalism in Western Europe that Wallerstein presents is therefore the outgrowth of a worldwide division of labor based on the imperialist expansion of Europe. The free labor of capitalism at the center depended on the organization of forced labor in the peripheral areas of colonial penetration. The result, however, was not the emergence of a new world empire but a variety of political entities at the center in Western Europe. (It is not clear to me how the case of the Italian city-state articulates with his basic argument.)

Of course, there are secondary elements in the argument. For example, the ecological perspective leads Wallerstein to examine the geopolitics of the nation-state. He argues that the West did not have to contend with direct invasion as did Eastern Europe or China, harassed by Japanese-based marauders. This favorable condition apparently contributed to the emergence of the world economy in the West. But the Ottoman Empire pressed heavily on Southern Europe and the Muslims penetrated deeply into Spain. Likewise, Wallerstein has only limited concern for the consequences of warfare within Western Europe. Of course, he attributes little or no importance to cultural values or even to the institutional structures of Western Europe.

Wallerstein's argument is pursued energetically, but I do not find the results compelling. He appears to be saying that the commercialization of agriculture does not account for the accumulation and transfers of capital required for the initial industrialization that occurred in Western Europe. Trade and the economic surplus accumulated from imperialistic practices were essential economic prerequisites. I would appreciate a more fine-grained analysis of the mechanisms by which the economic results of imperialism were transformed into the economic agencies of sustained internal economic growth. (I prefer "imperialism" to "world economy.") But the thrust of his argument does not accord with the documented historical pattern.

Adequate research not only compares Western Europe and Eastern Europe but also compares the patterns of economic expansion in the various nations of the West. Portugal and Spain were the pioneers in effective imperialism and capital accumulation, but they were hardly the front-runners in the emergence of a modern capitalist economy. Wallerstein seems aware of this relationship. The locus of economic expansion shifted to the Netherlands and then to England and France.

I remain more impressed by Schumpeter's reasoning that the economic products of imperialism were probably irrelevant to the emergence of capitalism, or, stated in more propositional terms, that the sociopolitical base of imperialism was found in those elements which were not part of the

process of capitalist development. In this view, the early imperialist accomplishments of Spain and Portugal would have inhibited capitalism.

Imperialism is not only or primarily an economic impulse in its origin; it is also a religious, cultural, and military movement. The social base of Western imperialism—to the extent that we have a sense of historical detachment for understanding this movement—is more than a desire for economic exploitation. This is hardly to deny its economic consequences or to overlook the fact that, as capitalism in later centuries became institutionalized, it was compatible with imperialist economies. But the social and political bases of imperialism were not the same as those responsible for capitalist expansion, especially for industrial development.

Thus, the more plausible argument remains to be assessed. Capitalism in Western Europe developed to the extent that imperialism and particularly domestic military institutions could become differentiated from the rest of the elite structure in such a way that the economic relations required for sustained economic growth could emerge. The Netherlands is the crucial example. There, I am informed by Maury Feld, the capitalist breakthrough, so to speak, occurred because a feudal aristocracy was not essential for military defense. Instead, the middle-class capitalists created a societal base that employed a mercenary army, subject to their control. Such an army did not block the development of capitalist norms. In other countries the aristocratic-military elements were barriers to such development and took longer to be controlled.

If one is interested in comparative sociology—that is, cross-national sociology—one must read or at least sample the recent outpourings of a new genre, cross-sectional quantitative studies of social and economic indicators that have been processed by factor and path analysis. The relief from this approach afforded by Immanuel Wallerstein is indeed commendable. There are an element of excitement and an overtone of spectacle in the sheer energy expended.

However, sociologists concerned with the history of ideas will be struck with the intellectual *chutzpah* in Wallerstein. How else can one characterize an undertaking which makes such a heavy use of the distinction between center and periphery and does not contain a single reference to the contemporary explicator of this idea, not even to refute his current usage? Historians also will find *chutzpah* in Wallerstein's efforts, since he implies that they have not been interested in the world system, in the interplay between European states, and in the influence of colonialism on economic development in the 15th and 16th centuries. In fact, historians will also complain that, despite the vast array of footnotes conspicuous on each page, Wallerstein's book is very selective in its coverage and emphasis of the available historiography. For me, it is particularly puzzling that there is not one reference to the powerful study by the British historian Henry Kamen that is a leading analysis of much of the same period. Kamen's *The Iron Century: Social Change in Europe, 1550–1660,* which appeared in 1971, evaluated extensively some of the central themes of the Wallerstein volume.

Sociologists will be especially interested in Kamen's effort. First, he handles the sociological literature on capitalist development, including Weber and Tawney, with considerable skill and detachment. Second, the interplay between empirical data and "theory" is energetic and highly self-critical. *The Iron Century* presents a collection of quantitative social and economic indicators that is striking and helps the reader obtain an effective overview of the patterns of social change. While sociologists develop theories of capitalism, it is indeed refreshing to hear from the historian that, "unfortunately, any serious discussion of capitalism must discuss capital, and there is [*sic*] almost no data in this period on capital formulation and growth" (p. 118). For most of the period under discussion, Kamen does not discuss the era as one of the development of capitalism but rather as a period of decay and disruption of the socioeconomic order that only later reestablished itself on the basis of monarchical authority and private property. Using the available data, he emphasizes the limited contribution of the profits of commercialized agriculture to the development of capitalism, commercial or industrial. Moreover, there is a body of convincing scholarship that points out that, in particular areas, the welfare of peasant sections of society was directly related to the prosperity and relative autonomy of the urban centers of the period. Kamen rejects the idea that the emergence of the capitalist entrepreneur rested on the bullion and items of trade from the colonial hinterland. In particular, Spain and Portugal, the pioneer colonizers (Spain was also the largest), failed completely to enter the process of capitalist accumulation. These nation-states were economically weak, and their reliance on the flow of precious metals hindered the development of industrial enterprise. In the end, we are back to the need to account for the rise of urban centers in northwestern Europe and England. We are back to the formulation of *Fragestellung* but not necessarily to the answers of Weber and Tawney and their accounts of the rise of the semiautonomous urban centers and the bourgeois elements that supplied the mechanisms for the accumulation of capital for the ensuing self-sustained economic growth of capitalism.

The manner in which Wallerstein poses the problem sets the intellectual pace of the study, of course, and limits the answers he develops. Is it sufficient in the present state of macrosociology to raise the question of societal transformation in strictly economic terms, to make economic growth the sole dependent variable? Barrington Moore, Jr., in his comparative analysis, is more explicit and comprehensive. Moore is concerned with explaining not only economic growth but also the conditions under which economic growth was accompanied by parliamentary versus authoritarian regimes. Wallerstein excludes these issues, along with those associated with the emergence of the idea of citizenship, the rights and obligations of the population, that set the context for the contemporary "crisis" of the modern welfare state. Uniformities and differences in political institutions and conceptions of citizenship are not at the heart of his analysis.

It is, of course, understandable that Wallerstein's volume will be compared to Barrington Moore's *Social Origins of Dictatorship and Democracy*.

This has been done by Michael Hechter, a student of Wallerstein's at Columbia University, who reviewed the study in detail in *Contemporary Sociology*. However, I disagree with Hechter and judge Moore's work to be a superior contribution. Moore is an elegant writer, clear and concise. He sets forth his problem with precision and his approach produces explanatory propositions. I agree with Hechter that in contrast Wallerstein's formulations are often imprecise and that the argument is diffuse and proceeds by indirection. Since Wallerstein's argument is frequently implicit, it may well be that I do not accurately grasp its development; but the manner of argument is one reason why his book is problematic. His conclusions would be more effective if he were more explicit about his point of view and how that point of view defines his problem. He also tends to insert ex cathedra pronouncements that are distracting: "Much of contemporary social science has become the study of groups and organizations when it has not been social psychology in disguise. This work, however, involves not the study of groups, but of social systems" (p. 11). And as an exercise in comparative analysis, we are treated to conclusions no more precise than, "In the sixteenth century, some monarchs achieved great strength by means of venal bureaucracies, mercenary armies, the divine right of kings and religious uniformity (*Cuius regio*). Others failed" (p. 157). Moreover, in his formulations are interspersed similes that are misplaced and hardly clarifying: "In the sixteenth century Europe was like a bucking bronco" (p. 356).

I also agree with Hechter that "There is a certain lack of conceptual precision which harms the analysis." In fact, and amazingly, Hechter concludes, "There is no theory to account for the triumph of the European world economy in the sixteenth century." If this is so, given Wallerstein's goal, what has he accomplished? Of course, I do not expect Wallerstein to produce a "theory of modernity." The underlying question is whether this bold and ambitious enterprise actually clarifies our understanding of large-scale historical change.

The crucial issue in the analysis, as in studies of macrosociology generally, is the model, the imputed process of causality, or the flow of antecedent conditions and subsequent results, that emerges at the end of the effort. Wallerstein, despite his ideal types, is not very clear about that which is being transformed or the mechanisms through which the changes take place. At best there is a tautological character to his argument, namely, that a set of economic conditions produces an economic outcome of necessity, and the associated process of sociopolitical change is only loosely linked to the economic context. His argument is weakened by the absence of a clear statement about that which is to be explained. His approach thus contrasts sharply with that of Barrington Moore, who is seeking to explain the different political outcomes of economic development. Thus, while Moore's study immediately became part of the corpus of comparative macrosociology, it remains to be seen what the precise importance of Wallerstein's undertaking will be. This is not to deny that Wallerstein struggles

heroically to pursue his research in scholarly fashion, although, of course, his world view influences the outcome.

If Wallerstein is read in his own terms and in the light of the available interpretive literature, he does not really demonstrate that a European world economy triumphed in the 16th century, even using the time period he explores. That triumph occurred at a later date. It could be more effectively argued that the development (I do not like the word "breakthrough") in European technology of firepower and sea power, plus the organizational procedures of the period, enabled Western European groups to launch what for the time were relatively large-scale imperialist and marauding kinds of enterprises, followed by colonizing expeditions throughout vast areas of the non-Western world. The economic results dramatically altered the economic relations of Western Europe, but these results are not to be construed as the central element in the emergence of Western capitalism.

At the end of his volume, Wallerstein has a biting appraisal of the idea of tradition: "It may be taken as a general sociological principle, that at any given point of time what is thought to be traditional is of more recent origin than people generally imagine it to be and represents primarily the conservative instincts of some groups threatened with declining social status" (p. 356). Wallerstein considers tradition to be a cover for privilege. I disagree. Are we dealing with a disguised effort to present a political ideology in the form of social science categories? Historical scholarship has underlined the fact that revolutionary leaders typically incorporate traditional symbols to legitimize their activities. I am struck by the emphasis revolutionary movements place on their traditional heritage—the French echoing of the Greek aspirations, for example. As a social democrat, I cannot think of progressive social change without referring to the vitality and relevance of hundreds of years of tradition in "social democracy": those intellectual, cultural, and institutional experiments which have enduring vitality regardless of their immediate operative or political success. Even more pointedly, the intellectuals of the third world who are searching for an alternative to the "world economy" clearly desire that social change should produce a result which recognizes their precolonial traditions and culture. The struggle to mobilize and refashion the symbols of cultural and political tradition, historical and contemporary, is not an epiphenomenon but a vital process that I trust Wallerstein will not overlook in the volumes ahead.

ECONOMIC AND SOCIAL DEVELOPMENT ON A EUROPEAN-WORLD SCALE

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Once in every hundred years, it seems, men have a desire to stand back from their handiwork and survey it in perspective. The urge comes upon

them when the economic and social problems that confront them at close quarters are more than usually difficult and they find themselves in a tunnel without much light at the end. In such circumstances, it is comforting to view things at a distance. This heroic exercise was attempted in the 1650s by James Harrington, in the 1770s by Adam Smith, and most impressively in the 1860s by Karl Marx. The psychological moment has again arrived. Barrington Moore gave us one panoramic view in 1966, Perry Anderson last year gave us another, and now Immanuel Wallerstein offers us a third.

Inevitably such large-scale reappraisals of man's economic, political, and social development encourage prophecies about the future. On his own admission, Professor Wallerstein undertook his task "in order to understand what alternatives lie ahead." So his view of the past helps to predict the future, although we must wait for other volumes before Wallerstein gives us his final considered view. But it points in a socialist direction—that much is clear—and no doubt sociologists and economists will judge it, in part at least, by its predictions.

Historians will judge this book in a different spirit. They concern themselves more with the shape of the past than the future. And they are, of course, pernickety about factual detail. Indeed, because of their concern to get the facts about the past absolutely correct, few are inclined to paint panoramic landscapes, while many are ready to deprecate such attempts by others. Among the latter, some will recoil even before they get past Wallerstein's introduction, for he warns them that he is presenting events "at a certain level of abstraction": it sounds ominous. Yet any suggestion that "abstraction" in this case means a certain carelessness in the treatment of evidence is false. Wallerstein has the surgeon's skills: he takes both the long view and the short, and in delineating detail he is both accurate and sensitive.

The Modern World-System, then, is a heroic and impressive achievement. Wallerstein has collected and digested a veritable encyclopedia of information, yet in condensing it all into a small compass, he does not lose sight of the finer points of interpretation, which are often the key to understanding the whole. Indeed Wallerstein alights on them with unerring precision. Another engaging quality is his solicitous care for his readers. He never marches ahead of them but makes sure that they understand his argument at every step. And to persuade them more readily, he does not simply summarize the views of others (thus laying mine fields across his own path); he quotes their works at some length in his text and in his footnotes. His footnotes indeed constitute a second book, incorporating a superb bibliography, some stimulating discussion of knotty points, and many supporting illustrations.

On many grounds, then, this is an exhilarating and satisfying book. But it will have a permanent niche in my memory for one particular reason: it explains more convincingly and sympathetically than anything I have read hitherto the *actual process* of economic and social development

on a European-world scale. I think I have perceived it in research concerning a much more restricted geographical area. Wallerstein sees the same process on a world scale. He convinces me that, whether one is attempting to see the interrelationship and interlocking development of relatively small economic regions of England, of European countries, or of continents, the process is the same. Economies, small or large, compete with one another, make demands on each other, and thereby drive each other to adopt new economic expedients having their own distinctive social repercussions. This constitutes the dynamic process of economic and social change. And the historian's herculean task is to trace and describe ever more exactly this perpetually spiraling movement, which carries distinctive regional economies forward along fresh paths, partly at their own choice, partly under the force of circumstances created outside them.

Thus at a local level I myself have studied a meat-producing region in the English midlands in the 17th century transforming itself under internal and external pressures into a dairying region. A new but assured market for butter and cheese was growing near at hand in industrializing towns like Birmingham This obliged other pastoral regions in the northwestern part of the country (much nearer Scotland than London) to fill the threatened gap in London's meat supplies by making a larger contribution to the national meat market. As soon as they made this move, however, they were drawn ever deeper into the national marketing network, for it offered them much more than a market for their store beasts. It involved them in a complex economic system that called on them to supply other goods: their copper, their salt, and an unlimited supply of handicraft goods. The south could absorb as much of the latter as the north could manufacture; the north, for its part. had many underemployed pairs of hands needing only the financial incentive to set to work. Northwestern England was first pulled and then pushed across a threshold and thereafter was joined firmly to the national agricultural and industrial economy. Before long its young men were seeking their industrial training in East Anglian clothing centers like Norwich and returning home to modernize the textile industry. Its landowners were learning from southern landowners and merchants how to set up new handicraft industries like linen and canvas making and how to establish saltworks. Its social leaders even developed an interest in Ireland, seeing the possibility of establishing between Whitehaven and Ireland the kind of economic relationship that London had with Cumberland. The economic interests of the Northwest now developed in several directions, strengthening ties with midland and southern England and forging yet new links across the Irish Sea. An isolated outpost of England, deemed heathen and barbaric in the 16th century, was such no longer. Indeed in the later 17th century, the Northwest was suspected of being a good deal more prosperous than southeast England and certainly bore a lighter burden of taxation.

Here is an illustration at a local level of the interdependence of regional economies, showing how one was transformed and another was

drawn for the first time into the mainstream of English economic life, learning thereby to build up new marketing networks in yet more far-flung places. Wallerstein, viewing the European world-economy, traces exactly the same process on a much larger canvas. He starts from the undisputed premise that European countries worked their individual economies into a truly world economy in the course of the 16th and 17th centuries. The development, as he correctly argues, was made possible by specialization, as one community concentrated on activities which it could perform well, at the expense of others that it performed indifferently. Thus Poland in the late 16th century came to concentrate on grain production in order to supply western Europe in general and Holland in particular with bread corn. Holland in its turn concentrated its efforts on vegetables, dyes, and textiles and was so remarkably successful in its undertakings that its economy was cited as a model for others to copy. And copy they did. Imitation had the effect of sharpening competition between countries, as they all took a hand in the same agricultural and industrial occupations. In the 16th century, for example, Holland made the best and cheapest pins and Flanders the best knives By the late 17th century, England claimed superiority in both skills. How had England improved its performance? By persuading Dutch and Flemish craftsmen to come to England and teach their skills to Englishmen.

This introduces the second important factor in the spiraling process of Europe's economies. Foreign expertise was being transferred from one country to another, moving with assurance from one congenial center to another potentially as favorable. Wallerstein cites the Genoese financiers who moved to Portugal and Spain and the Jews who moved from western to eastern and southern Europe. Descending to a lower level, one could cite the woad growers who moved from Languedoc to Hampshire, the glassmakers who moved from Normandy to Sussex, the saltmakers who moved from Holland to Yarmouth, and the fen drainers who moved from Italy to Holland and France and thence to England. The flow of skills in every direction across Europe was like the flow of many small, seemingly wayward streams which yet could and sometimes did merge into an impressive river. The migration of relatively few craftsmen from one country to another, positioning themselves in the right places (and there was nothing haphazard about the choice of their new home in a foreign land) could raise the industries of their adopted country to a much higher level of expertise, turning them into vigorous competitors with all comers.

An exacerbated commercial war threatened in the 17th century. "All trade [is] a kind of warfare," declared Sir Josiah Child. How was suicidal war averted? As Wallerstein explains, certain major changes in economic circumstances took place on a European scale. Population and price movements conspired to slow down the rate at which enterprise could expand and production increase. The alternative course on which men then embarked was diversification and differentiation. Industries producing the same goods offered them in different qualities, at different prices, to suit

the purses of different classes of customer. Differentiation, indeed, saved many regional economies from serious depression and even raised some of them to greater heights of prosperity than ever before, if they happened to set their sights on the classes with the most spare cash. This did not necessarily mean the wealthy gentry or middle class but often the wage laborers whose spending power increased markedly as the value of their wages rose.

Differentiation is most easily identified at the local level when one can actually compare the prices of a yard of linen or the cost of four ounces of pins. Wallerstein does not have the most effective illustrations at this point. But he has the facts of the case: he contrasts the monoculture that continued successfully in outlying colonial and semicolonial territories like Spanish America and Poland with the differentiation in farming and manufacturing which marked economic progress of the more advanced "core countries." In some passages, it is true, it seems as if Wallerstein thinks this a backward step: "a reversion to handicraft industries," he calls it when describing developments in southern Germany (p. 187). In fact differentiation within domestic industry was promoted by this reversion to family enterprise. Factory-based industry was much more likely to produce and persist in producing stereotyped patterns Family-based industry fostered flexibility in style and price which preserved the health of many industrial regions and built up their resources for the future. Such regions were often the seedbeds of the Industrial Revolution. But we must wait for Wallerstein to pursue this later story in his next volume and give his interpretation more clearly.

Many more issues are raised in this masterly survey than can be discussed in this review essay. Much space is given to the political framework within which different economies, with their distinctive social structures, pursued their livelihood. Forsaking for a moment the role of historian, I join Wallerstein in wondering, at the end of it all, "what alternatives lie ahead." If we see the dynamics of change in the complex interrelationships of small units cooperating and competing with each other, then Wallerstein's superbly rich illustrations of this process underline the inventiveness and ingenuity nurtured thereby. Every regional economy facing the challenge could find a different solution, some more successful than others. This seems to be the cogent, logical argument in favor of the devolution of power by central governments nowadays. Such agitations as those urging the English Parliament to give Wales and Scotland independent assemblies could draw much ammunition from this store of historical experience, for as Wallerstein expresses it, "multi-layered complexity" provided "the underlying turbulence that permitted technological development and political transformations" (p. 86). By promoting diversity, we permit more numerous alternatives to be tested, whereas the drive for centralization and uniformity which afflicts us at present reduces the number of alternatives and doubtless casts out untried some successful solutions. The historical evidence of Wallerstein's book proves one thing

conclusively, that no human being, however skilled in the arts of prophecy, could say with assurance how many alternative courses may be devised in a given situation or which will ultimately prove most fruitful. It may be that Wallerstein will not in the end find the answer he seeks.

Book Reviews

Conquest and Commerce: Spain and England in the Americas. By James Lang. New York: Academic Press, 1975. Pp. vii+261. $12.95.

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The volume of literature dealing with the colonization of the Americas is immense, yet surprisingly few scholars have attempted a synthetic study dealing with colonization in a comparative context. Those comparative studies that do exist, such as Louis Hartz's collection of essays, *The Founding of New Societies* (New York: Harcourt, Brace & World, 1964), have tended to be based on shaky theoretical and empirical foundations. James Lang, who has confined his comparative analysis to the activities of Spain and England in the New World, has probably come closer to integrating the necessary mastery of the substantive literature on colonization with a plausible interpretive scheme than anyone who has preceded him.

Lang contends that the radically different schemes of bureaucratic organization imposed by Spain and England on their overseas colonies produced fundamental differences in the character of the provincial ruling classes and institutions of government that emerged in the mature colonial societies of Spanish and British America. The Spanish, according to Lang, sought to introduce a comprehensive set of institutional controls designed to regulate not only trade and conquest, but all aspects of the religious and secular life of their overseas colonies as well. The English, embarking on their colonizing efforts at precisely the time when the unitary authority of the state was divided by struggles between the king and Parliament and by factionalism within Parliament itself, were both unwilling and unable to impose an overarching structure of authority over their colonies.

This view of an imposing Spanish bureaucracy and of a relatively lax English system of control is hardly novel, but Lang's discussion goes beyond a mere enumeration of the formal institutional powers of the two colonizing nations. Incorporated into that discussion is a competent survey of the development of agencies of provincial power and status within the New World. The Spanish, under the Hapsburgs, managed to achieve a delicate balance between the interests of the crown and those of influential creoles. While that Hapsburg bureaucracy was committed to the regulation of virtually all aspects of the commercial, financial, political, and religious life of the Spanish colonies, it was composed nevertheless of both royal officials and provincial leaders. The Bourbon reforms of the mid-18th century—the introduction of the intendancy, the move to control mining revenues more closely, the expulsion of the Jesuits, the weakening of the powers of the creole class generally—served to destroy this delicate balance between royal and provincial power. Napoleon's invasion of Spain

may have precipitated the collapse of the Spanish empire, but the bureaucratic instability within the American colonies caused by the Bourbon reforms was a fundamental cause of that collapse. Because of the weakened position of the creoles in the bureaucratic structure of the empire, the movements toward independence in Spanish America were fragmented, proceeding from *cabildo* to *cabildo,* with little central organization capable of transforming the overthrow of Spanish authority from that of a series of local civil wars to a unified independence movement.

The evolution of the provincial elite in English America, by contrast, moved in almost precisely the opposite direction. Initially weakened, especially in the southern colonies, by the lack of either the economic power or social prestige that were the normal accompaniments of political power, the provincial elites of British North America had by the 18th century acquired a legitimacy and a base of institutional power—the assembly—that enabled them to maintain considerable autonomy from the dictates of an already lax British colonial bureaucracy. The English colonial reforms following the Seven Years' War, like the Bourbon reforms, served to threaten that autonomy, but the provincial elites in the English colonies had by that time achieved a personal standing and an institutional base lacking among the creole leaders in Spanish America. Thus, whereas the independence movements in Spanish America were delayed by a half century after the introduction of the Bourbon reforms and, once effected, remained highly fragmented, the decision for independence in British America was both relatively swift and unified.

Lang has constructed a plausible explanation for the differing character of royal and provincial bureaucratic power in the Spanish and English colonies, a substantial contribution in itself. Any study, however, which attempts to make coherent such dramatically different colonizing experiences inevitably runs the risk of oversimplification. By focusing primarily on the formation of royal and provincial bureaucracies, for example, Lang slights other factors which dictated the different characters of North and South American societies. While he touches briefly on the process by which Spaniards and Englishmen exploited the labor of the Indian and African portions of the colonial populations, that discussion is aimed primarily at buttressing his main argument relating to the establishment of royal and provincial bureaucracies and not at illustrating the impact the presence of those populations made on the social order as a whole. The very existence of a sizable, semiliberated aboriginal population in Spanish America, the virtual extermination of the Indian population in British America, and the creation of a sizable, unfree African population in the southern portion of British North America were all factors that influenced not only the conduct of the various royal and provincial bureaucracies but also the very essence and regional character of each of those societies.

Similarly, Lang's grasp of the historical literature on Spanish and English America, while generally impressive, occasionally falters in his discussion of developments in the British colonies. In particular, his analy-

sis of the transition in Virginia from a servant- to a slave-based economy would have been improved by a familiarity with Edmund S. Morgan's recent work on colonial Virginia. And Lang's contention that "the establishment of the slave system in Tidewater Virginia was accompanied by an exodus of independent small planters to the frontier or to other colonies" (p. 125), while it supports his thesis relating to the development of a stable, slave-based elite, seems to me to be plainly incorrect. For the most part the independent, small planter remained an integral part of Virginia society; many of those small planters undoubtedly supported the deferential, slave-based social system created by the gentry, but, as Rhys Isaac has recently demonstrated, many of them probably remained resentful of the dominance of the gentry classes, creating a potential for instability and social conflict that Lang generally ignores.

These weaknesses, together with an organizational format that forces Lang to discuss the English and Spanish colonizing experiences in two wholly separate sections of the book, all serve to make this study a starting point for further comparative analysis rather than a definitive treatment of colonization in the Americas. Lang's book is, however, the most promising point of departure yet to come along.

Social Policy: An Introduction. By Richard M. Titmuss. Edited by Brian Abel-Smith and Kay Titmuss. New York: Pantheon Books, 1974. Pp. 160. $8.95.

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This book is a collection of lectures from an introductory course given for many years at the London School of Economics by Richard Titmuss and presented for the last time while he was dying of cancer. After Titmuss's death in April 1973, the book was assembled and edited by Brian Abel-Smith and Kay Titmuss. Any posthumous publication on the initiative of one's colleagues bears testimony to the person as well as the scholar. It is thus appropriate that this book may reveal more about the author—his premises and predilections—than about social policy itself.

The book portrays a scholar of simultaneous rigor and compassion seeking a framework for assessing social policy. Social policy itself is conceived as purposive action, change, and problem solving in the general direction of "progressive redistribution and command-over-resources from rich to poor" (p. 29). As evidenced in Titmuss's monograph on comparative systems of blood donation, *The Gift Relationship* (London: Allen & Unwin, 1971), his scholarly precision and political sophistication never eclipsed his code of personal decency and unabashed altruism. Indeed, Titmuss's own orientation to social policy stressed the government as the agent of altruism rather than as its opponent. Titmuss was very much a governmentalist in his views on welfare and social change. In these days of

antigovernmental cynicism, such conceptions may seem both antiquated and naive and the product of British noblesse oblige rather than the result of an objective social science. Surely, all of these characteristics were involved in Titmuss's work to some extent. But Titmuss was more than an alien curiosum. His work had a distinctive tone which has begun to resonate increasingly in American sociology.

Certainly Titmuss was aware that not all government policy is sound, that both politics and bureaucratization can produce "ill fare" in the name of "welfare," and that this is sometimes intentional as well as unintentional. Titmuss viewed social policy as fundamentally a matter of value choices. In fact, he stressed the rigorous analysis of social policy so as to expose hidden prejudices, assess stated objectives, and even expand limited horizons. After citing Joan Robinson's statement that " 'Without ideology we would never have thought of the questions,' " Titmuss went on to say, "And so it is with social policy models which, with all their apparent remoteness from reality, can serve a purpose in providing us with an ideological framework which may stimulate us to ask the significant questions and expose the significant choices" (p. 136).

Titmuss offered three particular policy models for discussion. First, the "residual welfare model" is one in which government and welfare institutions only come into play when the private market and the individual's personal resources break down—a system of temporary relief. Second, the "industrial achievement-performance model" deploys governmental and welfare policies to reinforce the free-enterprise economy by prodding more work, greater motivation, higher productivity, etc.—the so-called handmaiden model. Third, Titmuss's own favorite is the "institutional redistributive model" in which government services and welfare become major integrative devices for the society as a whole, providing universalistic responses to nonstigmatizing needs in the direction of resource redistribution.

In exploring these models, Titmuss surveyed a variety of alternative social service mechanisms. For example, the book includes chapters devoted to the (dim) prospects of achieving resource redistribution through the courts and private insurance, respectively. In addition, it offers a lengthy case study of British pension plans, contrasting the radical proposal of the 1968 labor government (never implemented) with the more traditional proposal of the 1971 conservative government (one that now forms the backbone of the country's present social policy system). As Titmuss described it: "The whole strategy of the Conservative Government looked to the past, to the myths and nostalgias of the 19th century; to a man's past, dominated by the virtues of individualism, actuarialism, risk-rating, saving, achievements . . . to a world in which you only gave to strangers through charity or the poor law" (p. 119).

In all of this, one finds the sort of ideological commitment which has been characteristic of British labor politics for so long. And yet Titmuss saw the commitment as more than merely ideological. While continually stressing the importance of value choices and even recognizing our ten-

dency to do the right things for the wrong reasons ("In Western society, we no longer believe in original sin; the concept of middle class guilt has partly taken its place" [p. 74]), Titmuss gently challenged us to seek a new level of discourse in the pursuit of more defensible commitments Social policies must not be evaluated solely in their own terms but rather must be understood in a far broader context. Titmuss lamented the common but artificial distinction between "economic" and "social" policy; he also urged more comparative assessments from an international perspective

In this latter connection, Titmuss noted that many American writers on social policy have been "extraordinarily parochial They generalize and develop sophisticated theories on the basis of American values and mythologies. . . . It seems the American middle classes (including many American academics) need scapegoats to sustain their values. And the welfare system is a scapegoat *par excellence*" (p. 45). Titmuss was especially critical of the recent work by Goffman and others on the "stigma" of welfare. But then he was also impatient with the entire functionalist tradition in Western sociology, one that posits a mechanistic equilibrium that invites a Sumnerian complacency on the grounds that policy intervention is neither needed nor effective. Titmuss's scholarly career was sustained on the premise that a marriage of scholarship and social policy not only can make a difference but also *must* make that difference. Perhaps it is this aspect of his work that will ring most peculiar in the United States. Here was a man genuinely concerned to make the world a better place—one who used, rather than excused, his scholarship to that end.

The book concludes with a postscript which is simultaneously poignant and upbeat. It is the last piece Titmuss wrote—a brief account for his students of his cancer treatment within the British National Health Service It includes portraits of several of his fellow patients and a description of the services provided them—first-rate medical care meted out both democratically and compassionately. More than another document in the Alsopian tradition, the piece underscores the irony that one whose scholarship had focused so favorably upon the National Health Service came to depend on it so critically at the end. The fact that Titmuss's scholarship was vindicated by his personal experience provided an ultimate gratification during his ultimate moments.

American Welfare Capitalism, 1880–1940. By Stuart D. Brandes. Chicago: University of Chicago Press, 1976. Pp. ix+210 $12.95.

James O'Connor

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In 1880 U.S. capital began its massive industrialization program, which was completed in the 1920s. Two or three decades earlier most of the conditions for industrialization had either been established or were rapidly nearing completion (international money market, railroad network, grow-

ing world market, integration of domestic market, accumulation of money capital, etc.). A missing ingredient, however, was the most important condition—the availability of cheap labor in massive quantities. Abundant opportunities for small-scale independent commodity production, especially in agriculture, slowed down the proletarianization of settlers and immigrants from northern Europe (the Irish excepted). The compromise between northern mercantalists and southern planters inhibited the development of capitalism in the South, hence the availability of free black and white labor for northern industry. Meanwhile, in eastern and southern Europe a huge land-poor and landless surplus population appeared, and since European capitalism could not absorb them, they emigrated to the United States. These foreign workers were the basis of American industrial capitalism (skilled trades excepted). The problem for American capital (which was similar to that facing the Japanese ruling class after the Mejii restoration) was two-fold: first, how to socialize foreign workers into disciplined American proletarians while keeping out labor unions, and second, how to maximize surplus value production by controlling the reproduction costs of labor power (that is, the cost of wages).

"Welfare capitalism" was the general answer: *American Welfare Capitalism, 1880–1940* documents in detail capital's largely successful attempt to organize the lives of its workers *outside* as well as inside the workplace. Largely innocent of political economic theory, Stuart Brandes's book is the kind of study without which no theory is possible: First, it documents in rich detail the process by which workers were Americanized in order "to bind workers to the jobs"; second, it documents the process whereby capital established direct control of reproduction conditions (hence the cost of wages) during a period when reproduction facilities were as scarce as mass laborers. Brandes shows that the concern of company-organized education was "to indoctrinate habits of thrift, simplicity, and economy" (p. 58) into the working class. He points out that capital lowered the cost of wages in company towns by building one church serving all denominations and mean row houses for the mass worker, while building more respectable quarters for skilled employees. In two brief, undeveloped passages Brandes recognizes implicitly that the essence of welfarism was control of the reproduction costs of labor power. In the closing pages, he writes that "the fact is that a society making heavy investments in capital goods cannot at once afford good housing, good schools, good medicine, and generous pensions for all its workers" (p. 147). In an early passage we read that each capitalist "had to manage an often sizeable labor force which could . . . become intractable and dangerous. . . . this involved more than just a question of wages. Business leaders found that workers had to be housed and fed and had other needs" (p. 4). But the author stops short of saying that welfarism was designed to control and define needs, thereby keeping wages low.

Brandes also shows that welfare capitalism was flexible enough to solve the problems of getting and keeping workers and controlling the costs of reproducing their labor power at the same time. Company towns, educa-

tion, health, welfare, and other programs permitted capital simultaneously to recruit workers to regions and sectors of the economy which were undersupplied with labor power and to maintain direct control of reproduction conditions. The fact that company housing was well above average in some sectors and well below average in others indicates the flexibility of the system. The author also suggests (but does not develop) the point that welfare capitalism was merely one aspect of capital's control of the working class, the other being economic crisis and unemployment itself. In crisis periods (for example, 1893–94 and 1921) capital cut back or postponed welfare programs which had been expanded during good times. Capital's formula thus was the big stick of unemployment in busts and the carefully measured carrot in booms. Seen this way welfare capitalism was an ideology of labor control, pretenses of humanitarian concern notwithstanding. The thinly disguised antiunionism which underlay welfare capitalism also indicates that welfarism was an ideology of control. Welfarism kept out unions in good times; unemployment kept out unions in bad times.

Welfare capitalism reached the height of its popularity among the capitalists in the 1920s. A 1926 survey of 1,500 large companies revealed that 80% organized at least one form of welfarism, and that one-half had developed comprehensive welfare programs. A Bureau of Labor Statistics survey, which concluded that welfarism cost capital no more than 2% of its total wage bill, suggests how sizable a return capital must have received on its small investment. Capital successfully controlled wages by planning needs and therefore was generally satisfied with the results of welfarism. Brandes attributes the decline of welfare capitalism in the 1920s (employee representation plans or company unions excepted) to its inherent paternalism, which the working class "found intrinsically demeaning" and hence either resisted or merely passively accepted. He also mentions the growth of automobile transportation, which freed workers from the company towns, schools, churches, and stores, as a factor. But the author only touches on what is perhaps the main reason for the decline and demise of welfarism, namely, that craft labor had been recomposed into mass work by the 1920s, and hence "industries in which success had come to depend on high-speed precision machinery rather than on a pool of skilled labor had little desire to protect the supply with welfare programs" (p. 141).

The author's overall evaluation of welfare capitalism is that it "met the human problems of industrialization in a way which at best can be termed only minimally acceptable. . . . Welfare capitalism was but a temporary expedient, an interim solution. It was better than nothing at all" (p. 147). But it has to be added that "nothing at all" and industrialization/proletarianization were mutually exclusive and logically impossible to combine. Capital used the resources necessary in order to reproduce the labor power of its workers, and no more.

Restraining Myths. By Richard Hamilton. New York: Halsted Press, 1975. Pp. x+296. $15.00.

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A restraining myth is a shared belief system with two components: it is empirically unfounded, and it has demobilizing effects on collective action aimed at change. The idea is a useful one and offers some promising possibilities. For example, it can potentially make more operational such ideas as the hegemony of a belief system and "false consciousness." It is also an idea with problems, some of which are not fully faced up to by the author.

Hamilton deserves credit for an ambitious book that confronts major issues in political sociology and has much to say that is sound and sensible about these issues. The major target of his argument is a body of related and still influential ideas that Hamilton groups under three headings: "centrist social science," "mass society," and "pluralist." His charge against this body of thought is that it is a set of restraining myths.

To make this charge stick he must accomplish two tasks. First, he must show that the empirical basis of the body of thought is weak or absent. His approach to this problem is through an extensive secondary analysis of survey data of the sort available in various data banks.

In general, Hamilton does a skillful and reasonable job in this secondary analysis. Using the assumptions of the arguments he is confronting, he suggests a number of reasonable expectations about the relationships in his data sets. Most of the expected relationships fail to appear or appear in a weak and unconvincing fashion. The empirical basis of the beliefs that Hamilton is challenging appears soft or nonexistent as charged.

The author makes a reasonable case that the belief systems in question are poorly grounded and reasonably deserving of the myth appellation. However, I should note that I leave something to be desired as an unbiased juror, being reasonably convinced of the verdict before I saw Hamilton's fresh presentation of the evidence. Other jurors, initially less persuaded than I was, will need to decide for themselves whether the evidence is convincing on this issue.

I am much more bothered by the virtual nonexistence of the second part of Hamilton's argument—that dealing with whether the beliefs are restraining or not. He does not really treat this issue as problematic but it is, and it deserves better treatment than given here. It will be easiest to illustrate this point with the familiar issue of "working-class authoritarianism."

Hamilton takes as obvious that the belief that manual workers have a lower commitment to civil liberties than do upper-middle-class people is restraining. It implies that those of a liberal disposition should let sleepings dogs lie. If the masses are aroused, their fury will not be easily contained and we will lose the baby with the bath. Hence, the implication of

this belief is restraining for those who would encourage the mobilization of workers for (say) a redistribution of income in society. Such mobilization efforts are too risky.

A belief in working-class authoritarianism can be used to draw the above implication, but it is not an inevitable implication of this belief. One might, for example, argue that a lack of concern for the niceties of the rules of the game on the part of workers increases their potential for collective action. Organizations engaged in political combat may be more effective if members are not overly fastidious about violating some of their opponents' civil liberties. Working-class authoritarianism could as easily be a component of a "mobilizing myth" as of a restraining myth.

To muddy the argument even more, Hamilton's proposed alternative borrows heavily from the body of thought being criticized and has its own restraining aspects. The author admits as much. "Only in the fairy tale does the elimination of the restraining myth give place to a non-restraining reality. The more likely alternative is a restraining reality" (p. 30). He claims his alternative interpretation is at least a bit more encouraging to advocates of mobilization for collective action.

At this point, the idea of restraining myths seems like nothing more than unnecessary name calling. If many of the arguments of a restraining myth could be made components of a mobilizing myth and if the reality may really be a restraining reality, then why employ the concept of restraining myth? Why not simply take on the task of showing that the beliefs in question have a weak empirical foundation. This is all that Hamilton really attempts in a serious fashion.

Hamilton's alternative belief system is labeled "group basis." Although he attempts to distinguish it from "pluralist theory," it clearly has a close kinship with this family of ideas. Hamilton emphasizes the rootedness of the political ideas of working-class and lower-middle-class people in primary group traditions. He is skeptical about the meaningfulness of intermediate associations in reflecting the interests and values of these subgroups. His emphasis, in contrast, is on the independence of the interests of these organizations from those of their alleged constituency.

If one has to choose on this point, then Hamilton's version seems considerably more restraining than the rival that he criticizes. Without the implicit name calling involved in the concept of restraining myth, this would be no special embarrassment to Hamilton, but under the circumstances it becomes one. In fact, the reality is that intermediate associations reflect the concerns and desires of their constituency in varying degrees—some reflect them pretty well, and some do not.

The relationship among the different beliefs that Hamilton is criticizing is also less well developed than it might be. The three positions he is criticizing and his own alternative are closely related, and the similarities and contrasts among them are poorly articulated. To take an example, pluralism and mass society beliefs are essentially part of the same larger belief system. If people's political attitudes are well anchored in primary groups and these are linked to the polity through intermediate associations

that have access to political power, then the primary conditions for mass politics are absent. If these features of the social and political landscape are not present, then mass politics is the result. The beliefs are two sides of a coin, not competing views. Proponents may disagree over whether a given society does or does not meet the conditions of a pluralist social structure, but the basic argument about what it is that produces a given type of collective action remains the same.

In sum, Hamilton does a service by subjecting pluralist thinking to a series of empirical tests on which it fares quite poorly. He does not use the concept of restraining myth effectively in this work, but it remains an interesting and potentially very important idea for the student of social change. To make the concept more than a gratuitous put-down of those with whom one disagrees requires more than Hamilton has done with it here.

Political Power in Poor Neighborhoods. By Curt Lamb. New York: Halsted Press, 1975. Pp. xxii+315. $19.50 (cloth); $9.50 (paper).

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University of Arizona

Poor neighborhoods generally are not politically highly mobilized, and such traditional organizational bases for mobilization such as churches and political parties are particularly ineffective, both in organizing the poor for action and in accomplishing political goals. Nevertheless, the potential for effective leadership does exist in poor neighborhoods, and in those neighborhoods where it has developed with some militancy it has produced some institutional change. The more successful strategies of mobilization are: (1) politically active radicalism that recognizes responsiveness on the part of those who can effect changes, (2) group solidarity based on cultural (e.g., ethnic) revitalization, (3) effective publicity for local problems and political actions, (4) protest and direct action, (5) committed local leadership, and (6) coordinated associational infrastructure. Ideological radicalism alone is ineffective, but combined with activism, information, and support for local (but not necessarily national) government, it is instrumental in social change.

This brief summary cannot do justice to the detailed conclusions drawn by Curt Lamb in *Political Power in Poor Neighborhoods,* if only by omitting such specific topics as the political position of the poor, black women, the influence of the women's liberation movement, types of community leadership, associations in poor neighborhoods, political attitudes in relation to demographic characteristics, and so on. Yet the thrust of the work is how to revitalize poverty areas politically for the purpose of institutional change.

The empirical evidence reported is a large and complex body of data gathered under the auspices of the Office of Economic Opportunity to

evaluate the Community Action Program: the Local Change Study. Many aspects of these data have been reported elsewhere. Neighborhoods were intended to be the units of analysis, but the author's varied interests led to analyses with several different types of unit.

The statistical analyses consist mainly (though not exclusively) of percentage distributions, bivariate percentage tables, and constructed indexes or typologies. This gives the work a more descriptive flavor than sociologists are currently accustomed to, but is in no way inconsistent with the interests of the author. To be sure, Lamb does discuss the operation of social forces underlying political behavior, but he is also particularly interested in describing the unique current conditions surrounding the attempt of people in poor neighborhoods to achieve social change through political action.

The body of data reported in *Political Power in Poor Neighborhoods* is vast and rich, and yet it reminds the reader again and again of the fragility of social science. The atmosphere in which the study was conceived and the data were collected was highly politicized. Many of the political questions to be answered by the study were the very bones of contention among the political figures involved with the OEO and the evaluation study. Government funds and agencies were used in asking political questions of "victims." One is not led to conclude that the data are "wrong," but that the study was seen by many respondents as well as many officials or politicians as a soccer ball in an important political match rather than as a source of intrinsically valuable information.

For all of the social-scientific issues confronted in this volume, the original intent of the Local Change Survey was practical in nature and it would be a mistake not to regard conclusions about public policy as paramount. As sometimes happens, this was primarily an evaluation study of an intervention that was never really fully implemented. Thus the bearing of the data on policy conclusions is a little problematic. If evaluation data show little effect, does one conclude that the treatment was ineffective, or that it had not been applied strenuously enough? If the latter possibility can be entertained seriously, then just what good are the data? In such a case, policy critics and supporters alike are left with "heads I win, tails you lose" debating material.

Nevertheless, *Political Power in Poor Neighborhoods* brings an enormous body of empirical evidence to bear on a number of questions that are central in political sociology. It finds that some of the traditional answers are no longer correct (if they ever were). The discussions of issues (and of the bearing of the data on them) are very good. This is a work that sociologists of politics ought to be aware of.

Taking State Power: The Sources and Consequences of Political Challenge. By John C. Leggett. New York: Harper & Row, 1973. Pp. xiii+533. $13.95.

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The dominant pluralist and consensus models of the 1950s were poorly equipped to deal with the eruption of student protest and urban riots on the domestic front and the proliferation of antiimperialist movements abroad which characterized the political environment of the past decade. Not surprisingly, recent years have witnessed an avalanche of literature on the state and class conflict, revolutions, and insurgency. Both orthodox Marxist and liberal theory have undergone substantial sophistication in the process, while the search for a general, more structural theory continues.

The present volume, which is largely a selection of Leggett's own essays sprinkled with contributions from such diverse authors as Maurice Zeitlin and Elman R. Service, occupies in several ways a place on the front lines of recent sociological reorientation: at once, it attempts a cross-comparative and a historical perspective in mapping out how revolutionary movements organize against the state, how they fail or succeed, and how specific weaknesses are inherent in all types of movements. Being very much part of "radical sociology," we might have expected a derivation of historical materialism in Leggett's theory. Instead, we encounter one of the more spectacular eclectic syntheses in the history of sociology: fragments of Marx, Weber, Rosa Luxembourg, Mannheim, and Trotsky are systematically woven into Sahlin and Service's multilinear theory of evolutionary potential. Hence, as societies become more differentiated, they also become more resistant to radical challenge. However, either due to imperialism, domestic exploitation, or often a combination of both, the oppressed will rise against a state which has come to lose its legitimacy. In other words, while it is a mastodont and complex volume, the general argument is straightforward: revolutions tend to occur in societies organized around the exploitation and oppression of certain groups, classes, or ethnic/racial minorities. Shockingly, in this massive synthesis, Lenin's work on state and revolution is barely mentioned.

To give historical illustration of his thesis, and in order to develop his typology of revolutions, Leggett devotes the first section to cover variants of revolutions in "primitive," agrarian, and hydraulic societies. The ideal type revolution which emerges begins with "prodromal conditions" (e.g., a sense of relative deprivation is growing) which will translate into "power seizure" by moderates and radicals. If the revolution is to succeed, accession of the radicals must take place. Finally, the real test of the revolution lies in the appearance of "Thermidor," which is the period of reaction against implemented radical changes. In this phase, obviously, the teachings of Trotsky and Mao are pertinent. This section of the book is highly informative and stimulating—in some ways the strongest part of the volume.

The remainder of the book deals with the much more complex problems of political challenges to the state in advanced capitalism and state organization of counterinsurgency and social control. Unfortunately, it is exactly at this point that Leggett sacrifices much of the comparative and historical perspective which made the first part so fascinating. Except for a few case studies, such as Fascist seizure of state power in Nazi Germany and women's liberation in China and the Soviet Union, both the conceptual development of fascist, communist, and democratic ideologies and movements and the large number of empirical studies presented mirror the general ethnocentrism and unfamiliarity with European ideological currents characteristic of American sociologists. Most clearly, Leggett's treatment of center-left-right divisions within democratic ideology and practice has little validity outside an American context. The second major parameter of revolutionary potential in advanced capitalism is obviously the development of a class consciousness. On this concept, Leggett provides us with a very good discussion, although it is somewhat overly eclectic. The major part of the last, and longest, section of the book, however, analyzes insurgency and counterinsurgency in the United States.

Leggett has personally been active in grass-roots and university movements. This is reflected in the selection of case studies which overwhelmingly deal with various aspects of the "new left." Taken individually, these readings are often exciting and of high analytic quality. Some appear less relevant, and at times little is done to help the reader realize they may be so. But, taken as a whole, radicalism in the United States during the 1960s hardly seems an adequate source of material on revolutionary movements aiming for state power in advanced capitalism.

Of course, these very movements have been scattered and disorganized and, as the readings convey, have also been abortive. This can be explained as the result of weaknesses in the movements (bad timing or lack of mass support), or it can be explained in terms of effective counterinsurgent organization. Leggett dwells on both possibilities, the latter in more detail. While very informative, the readings on counterinsurgency suffer a problem inherent in Leggett's (and many other sociologists') theoretical perspective: there is a tendency to elaborate long lists of corporate state interlocks, personnel on various boards, and inventories of police and military armories as evidence of the validity of a "power elite" type thesis. Shared with a number of other left-liberal and Marxist sociologists, a preoccupation with the instrumentalities of social control tends to substitute for a structural theory of the state and its relationship to class and group conflict. Ironically, while such authors vehemently attack pluralism and the notion of the state as a neutral grab bag for those who participate, they still tend to conclude that it is a grab bag. Only the ruling classes have sufficient power to insulate the state from mass participation.

The critique here is, perhaps, too hard. But it is directed at a whole literature in political sociology, not at Leggett's book alone. Consequently, I may have obscured the fact that I find it an extraordinary piece of work:

highly readable, in general well organized and edited, passionate, scholarly, and at times approaching the highest standards of sociological description. Those who seek a more adventurous and stimulating undergraduate instruction will find this undogmatic reader a challenge to the critical aptitudes of teacher and student. Fewer student complaints about lack of relevance and conceptual sterility should be guaranteed.

Ethnicity in the United States: A Preliminary Reconnaissance. By Andrew M. Greeley. New York: John Wiley & Sons, 1974. Pp. ix+347. $11.95.

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It is a bit startling, more than half a century after *The Polish Peasant*, to have a "preliminary reconnaissance" on ethnicity come out of Chicago. The redundancy in the subtitle gives a hint of the exaggerations to follow. I have never quite understood why a few writers feel obliged to deny either the existence or the good sense of their predecessors. Rather than recognizing that they are "standing on the shoulders of giants," they give the impression that they are standing on the necks of pygmies—a phenomenon that I hope some sociologist of science will explore.

Perhaps what Andrew M. Greeley is saying by indirection is that survey research methods using fairly large national samples are only beginning to be used in the study of ethnicity. This is true, as it is true of many other fields of inquiry. Until those methods are improved beyond the level of this book, however, I will take Thomas and Znaniecki. Perhaps Greeley is also saying that there has been insufficient recognition of the degree to which American Catholics of Irish background have climbed not only the occupational and educational ladders but also the political ones. This may also be true, although certainly less true since 1960, to note an arbitrary but decisively important date for American consciousness raising in that regard. An additional, partly latent, message that Greeley is sending is: I am angry that various white ethnic groups have had to bear the major burden of recent social changes; and for their troubles they are blamed for the turmoil associated with those changes. There is much truth to this also. As I would put it, our stratification system, largely independent of individual intentions, places the costs of major changes on the relatively less powerful: *plus ça change* . . . . It is unfortunate, however, that this point is part of a hidden agenda in a book presumably dealing with a rigorous study of the influence of ethnicity on American society.

Greeley's book can be read on three levels. I got a little winded dashing up and down from level to level, but doubtless the exercise was good for me. It is, as I have already suggested, a moral and political statement. Since the moral and political issues he is dealing with are of great importance, related to justice and integrity in a diverse society, one can readily applaud his interest in them. I only wish he had not handled them pri-

marily by innuendo and exaggeration in a style that interferes with clarity of thought and reasoned debate. I kept thinking of an essay by Wayne Leys and wondering how Greeley would handle the issues if he had adopted Leys's mode of ethnical analysis.

The second and third levels are those of analysis of survey data and the development of a prospectus for further study. Greeley uses a wide range of survey materials, from the National Opinion Research Center, the Survey Research Center, the Current Population Survey, and the Harris and Gallup polls. These furnished different kinds of information about the respondents, but the author is well aware of the possibilities and the problems in these kinds of data. He demonstrated effectively if not definitively that there is an ethnic factor—separable from class and religion—influencing political, familial, and other attitudes and behavior in contemporary America. If the study is not definitive, it is partly because the data were gathered for various purposes and were not rich enough to permit the testing of complex hypotheses. In my judgment, however, there were also methodological weaknesses. Perhaps the most serious is Greeley's definition of ethnicity: One's ethnicity is the answer to the question, What is your nationality or background? Or, where do most of your ancestors come from? If an answer did not come readily, in the NORC surveys at least, respondents were pressed for an identification of their ancestry. If more than one nationality was given, the father's was used as the person's ethnicity. At various points (e.g., pp 155 and 310), Greeley recognized that origin and identification are not identical. I would go further and say that a full sociological ethnicity implies: self-identification, identification by others, and participation in associations built around ethnicity. With only a weak measure of the major variable, only tentative statements are possible.

Class, education, religion, and region were frequently used in an effective way as controls. (One has to be alert, however, to the fact that a table without controls may be followed, several pages or a chapter later, with controls added.) Of course, one can always wish for more refined measures. Region, for example, may be quite a crude control if no mention is made of state of birth. It is probably no longer true that "Detroit is the largest southern city in the country," but it is certainly true that knowledge of origin as well as of current address is necessary for adequate control of the regional factor.

Greeley makes good use of multiple classification analysis (MCA). I miss, however, some statement about problems of interpretation, especially when subsamples are small. In chapter 6 he reports that political participation by Jews is below average when class and region are controlled—a correct reporting of the MCA. But this requires some interpretation, for MCA, like other regression techniques, can take integral complexes apart. This finding says simply that if "southernness" and "low incomeness" were influencing Jewish political participation, and influencing it on the average in the same way they influence other groups, then participation would be below the national average. I am partial to MCA myself, but

would not want to be assigned the affirmative side in a debate: "Resolved, that if Jews were low-income southerners, they would have low political participation."

There are other methodological issues that require careful attention on the part of the reader. For example, Greeley finds that Americans of German Catholic background are more like Americans of German Protestant background on many measures than they are like Americans of Irish Catholic background, thus indicating that "ethnicity" is more closely associated than is religion with various personality and political variables, despite what he regards as "the conventional wisdom" to the contrary (see chap. 5). (One must wonder about the separation of religion from the cluster of factors that make up ethnicity, but I shall not examine that here.) Could it not be that Americans of German descent are alike, despite their religious difference, in both being nearer the mean of the American scores as a whole, thus showing their acculturation rather than their ethnicity, while Americans of Irish Catholic background are farther, on some measures, from the mean? The latter, with their somewhat more liberal attitudes and values, have been shaped by their long sense of opposition to English domination of Ireland. In this regard, their experience has something in common with that of Jews—an experience that may help to explain both the liberalism and the social mobility of the two groups.

Problems are also raised by Greeley's reliance on a limited part of the range of interpretations of the cultural sources of American ethnic groups He draws heavily, for example, on Edward Banfield's interpretation of southern Italy's "amoral familism," despite the sharp criticism of this view in the work of Sydel Silverman, William Muraskin, and others and despite the substantial literature that challenges the "culture of poverty" concepts on which Banfield's work is based.

Finally, to turn briefly to the third level of the book, all students of ethnicity will welcome Greeley's summary of the kinds of research now needed. He emphasizes that the book is a preliminary reconnaissance (in practice if not in etymology there may be other kinds of reconnaissance), and in his scouting of the territory, he has found several valuable routes to follow (see pp. 312–15 and 321–23). It is to be hoped that he and others will take up the suggestions for such studies as those dealing with differential paths of acculturation, the experiences of different waves of immigrants from "the same" society, the crucial influences of the family on ethnicity, the effects of ethnic intermarriage, and various forms of cross-national research. Perhaps Greeley's enthusiasm and his energy, if not his precision and his tact, will encourage the rest of us to press ahead with the study of these important questions.

Black Racial Attitudes: Trends and Complexities. By Howard Schuman and Shirley Hatchett. Ann Arbor, Mich.: Institute for Social Research, 1974. Pp. x+157. $10.00 (cloth); $5.50 (paper).

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This short but important work by Schuman and Hatchett has the distinction of being one of a very small number of monographs focusing entirely or substantially on black adult attitudes. It was not until the early 1960s that the first large-scale national survey of black attitudes was carried out by Louis Harris, and since then one is hard pressed to find any large-scale or significant work on black adult attitudes apart from D. R. Matthews and J. W. Prothro's *Negroes and the New Southern Politics* (New York: Harcourt, Brace & World, 1966), Gary Marx's *Protest and Prejudice* (New York: Harper & Row, 1967), and Leonard Goodwin's *Do the Poor Want to Work?* (Washington, D.C.: Brookings Institution, 1972).

The present work has a number of significant features. First of all it takes the position that "black attitudes, beliefs, and actions are of considerable importance, and deserve the same descriptive and analytic attention as do those of whites" (p. iii). This is a refreshing orientation when compared to the crisis response of much of the attitude research following the so-called riots of the 1960s, research which has doubtful generalizability. Second, it is a study of black attitudes over time, making use of three separate data sets: the National Advisory Commission's 15-city study carried out in 1968, a Detroit area study of blacks in 1968, and a broader Detroit area study in 1971. While the authors were forced to face the usual problems of comparability (see, for instance, Herbert H. Hyman, *Secondary Analysis of Sample Surveys* [New York: John Wiley & Sons, 1972], chaps. 6 and 7), they do demonstrate the usefulness of secondary analysis at a time of scarce resources for large-scale surveys. A cross-sectional study of attitudes is interesting enough to deserve the great effort expended by so many. An even more intriguing issue is the question of the stability of attitudes and the factors which affect their existence and change over time. From the latter perspective, the many studies of black attitudes carried out during the 1960s (especially those from ghetto areas), though interesting, may tell us very little of the attitudes of the broader black population, or even of the ghettos at another point in time. It is to the credit of the authors that they have not only attempted to study black attitudes over time, but selected points in time that span both the 1960s and the less turbulent 1970s. Finally, the authors are scrupulously honest about the limitations of their data and meticulous in their methodology. An aspect of the last point is their careful attention to possible interaction in their data, a practice that is far too uncommon.

Although Schuman and Hatchett do not state their objectives beyond

a very general "we tried to present a modest but complex set of data gathered using attitude sample survey methods, and to do so within a relatively objective framework of analysis and reporting" (p. iii), it is possible to detail a number of more specific tasks attempted. Briefly these tasks are the following: (1) to select a number of attitude items from the three above surveys that are replicated over time; (2) to analyze these items for change in black attitudes as a result of the assassination of Martin Luther King, and for changes between the years 1968 and 1971; (3) construction and testing of an index from the selected items; and (4) exploration of some methodological problems such as bias arising from the race of the interviewer. In most cases, the scope of the analysis is limited to the city of Detroit.

Thirteen items were initially selected, six of which are common to all three surveys. It is these six that are examined for changes in black attitudes as a result of King's assassination. Here the authors draw a blank, finding no real indication of attitude change in their data. While this might at first seem surprising, the answer may simply be that the riots that accompanied that event were the expression of already existing pent-up emotions rather than a manifestation of change.

Eleven of the 13 items are interpreted as "directional questions" (p. 13) that tap dimensions of black consciousness, alienation from white society, and militancy. When analyzed for evidence of change over the three-year period, 1968–71, six of these items reveal a shift that is significant beyond the .05 level. This is interesting, though by no means exciting, and the authors seem to have considerable difficulty making sense of the "behavior" of some of their items. The authors are aware of the problems of single-item analysis and "the hazards of taking marginal percentages as meaningful in themselves" (p. 21), and it turns out that this part of their analysis is preliminary to the construction of an index. The remainder of the book (beginning with chap. 3) centers upon the construction and use of this index.

This effort, though it too has its problems, proves much more fruitful. The 11-item index, called "Alienation from White Society" (p. 33), does reveal some shift in attitudes between 1968 and 1971—from a mean score of 3.85 to 4.35, or from 4.04 to 4.72 when data from only black interviewers are considered. Both are significant at the .001 level.

The search for correlates of the index in chapter 5 fails to turn up much. Age and education account for "4.4%" and "1.4%" of the variation in the index, respectively (pp. 56, 58). A painstaking massage of their data fails to find any relationship between sex, occupation, income (which just reaches significance at the .05 level), and a number of other variables and their index.

I found chapter 6, "The Alienation Index and Other Attitudes, Beliefs, and Actions," much more convincing, although at this point the authors candidly admit they were engaged essentially in "an effort at construct validation" (p. 77). The effort bears fruit, and they discover a large number of racial and nonracial attitude items that are impressively related

to their index. At the same time, their analysis touches upon a number of attitudes which show no relationship to their index. The conclusion they draw from the last point is worth emphasizing. They note that "it seems clear that scores on the Alienation Index—or probably any measure of racial attitudes—important as they are, do not determine or reflect black attitude in *all significant areas* of life" (pp. 114, 115; my italics).

If this work had to stand on the strength of its "findings" alone, it might be considered weak. In spite of the authors' repeated recourse to the "significance" of many of their findings, I was not overly impressed by the strength of these relationships (the old significance controversy). This ambiguity of outcome is probably due to a weakness in their index, a weakness which Schuman and Hatchett are keenly aware of and express in a number of ways. They remark, for instance, that "the merely moderate reliability of the index is due not only to the varied content of the items, but also to their varied format" (p. 32), and caution against "simplistic assumptions of unidimensionality" (p. 123).

Another problem faced by the authors was the sample size, which sometimes proved too small at crucial points (p. 71), and the limitation of the sample to Detroit. This limitation might be contrasted, for instance, with the companion volume by Angus Campbell, *White Attitudes toward Black People* (Ann Arbor, Mich.: Institute for Social Research, 1971), which was able to draw upon four national samples with numbers of 1,558, 1,536, 2,945, and 832, respectively. Unfortunately, this problem of sample size is one that limits most research on the black population, since even the most reliable national samples, such as those by NORC and the Center for Political Studies at Michigan, contain an insufficient subsample of blacks for extensive national analysis.

This study, however, has far greater importance than simply its findings. It serves as a model work in the research on black attitudes, not only because of the perspective taken by the authors, but also because of their careful methodology and the general sense of the uniqueness and complexity of black attitudes they reveal. The few pages contrasting trends in black attitudes with those of whites (pp. 124–28) are gems that should be read and reread by anyone who is prone to view black attitudes as merely variations of or "deviations" from white attitudes. The evidence they present on the strong effect of race of interviewer when the subject matter is racial or political is convincing. It is a finding that is probably applicable to any situation in which a characteristic of the interviewer contrasts significantly with that of the respondent, or to material that is very sensitive in nature. Those who study sex roles should take note. Whether or not this alienation index merits replication (given its present weaknesses), as the authors suggest, is not a matter that can be easily decided. I am inclined to feel that they have rather demonstrated the need for a more thorough study of black attitudes (racial and nonracial) than is possible through a weak index constructed out of individual items. This is a good beginning, but only a beginning.

Black Consciousness, Identity, and Achievement: A Study of Students in Historically Black Colleges. By Patricia Gurin and Edgar Epps. New York: John Wiley & Sons, 1975. Pp. xiv+545. $13.95.

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This book is about the aspirations and evolving ideologies of students in historically black colleges—a remarkably understudied group. That alone makes it a welcome addition to the literature of both the black experience and of contemporary college experience. But it is an important book also for social psychologists, because it documents the process of personal and group change during the turbulent 1960s (albeit within a specific context). Finally, it is a book which has much to contribute (from a base of empirical research) to needed changes in educational policy.

Patricia Gurin and Edgar Epps report in the preface that they began their research in an attempt to study the impact of the southern student movement on the activists who spearheaded change in the South during the early 1960s. It is indeed fortunate that they decided to broaden their focus to include the experiences of the general population of students attending the schools from which the movement sprang (p. v). The results reported are from a series of complementary studies, some longitudinal and others cross-sectional, whose sample sizes ranged from 200 to 3,639 and spanned the years from 1964 to 1970. The data were gathered at 10 historically black colleges (not identified in the book), carefully chosen to be broadly representative of such schools and to take account of institutional variables relevant to a study of achievement orientation (e.g., rated quality of school) and activism (e.g., degree of documented constraint in response to student and faculty activism).

A central question provides the basis for the structure of the book: "How did students integrate their collective commitment as Blacks with their goals for individual achievement and personal fulfillment?" (p. 385). The first of three major sections presents chapters on educational and occupational aspirations; the second, roughly parallel in design to the first, focuses on black collective commitment reflected both in attitudes and participation; the final section integrates the results of the first two and provides results of still other analyses. Cumulatively they are used to answer the question of resolving the conflict between personal and group commitment.

There are a number of general features of the presentation of results which are noteworthy. First, is the clear and full presentation of data on the characteristics of the populations and the schools. Their findings so clearly attest to the heterogeneity of schools and students that henceforth it will be ludicrous to attempt stereotypic characterizations such as "the student in black colleges." Second is the frequent and appropriate references to data from previous reports using larger samples. This device helps the reader to gain a better perspective on the study's population by

demonstrating their differences and similarities when compared to other black and white students. A third general feature of the presentation is the frequent attempt to apply the results. The authors, in discussing their findings, do not hesitate to conclude with straightforward suggestions on educational policy and prescriptive advice for faculty and administrators. Several of the policy suggestions are directed toward greater access to higher education for the poor. Their findings indicate quite consistently that the poor students were remarkably similar to the more affluent on the variables relevant to college success. At the level of advice to college administrators and faculty, Gurin and Epps point out that the most broadly successful students were those who had most individual contact with faculty and had a wide range of campus experiences. Whether the institutions have the wherewithal to implement such suggestions is another question.

In this generally balanced treatment of an important and sensitive topic I have found only one aspect of the presentation which causes me any serious misgivings. The authors seem to unjustifiably relegate pre-college experiences to an insignificant role. The importance attributed to current experiences has been well documented, but that need not in itself imply that earlier experiences were not important. I think that it was a mistake to use the term "socialization" when discussing family background variables, because one could easily get the impression that socialization itself was at issue or that the authors consider aspects of family background synonymous with socialization. Family background variables, such as parents' education, should be considered indirect and very inadequate indicators of socialization. Moreover, there is simply too much variance unaccounted for by the five background variables (a very small number indeed, considering the prominence given to the discussion of them).

Still, Gurin and Epps, two committed scholars, have given us a book of great value for policymakers, educators, and social scientists.

Middle-Class Blacks in a White Society: Prince Hall Freemasonry in America. By William Alan Muraskin. Berkeley: University of California Press, 1975. Pp. xi+318. $14.95.

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William Muraskin begins his illuminating study of Prince Hall Freemasonry, the largest and the most important secular national black fraternal organization in the United States, by stating that he wishes to "view the black bourgeoisie in greater detail and with more clarity than has been done before" (p. 1). In this attempt he succeeds admirably. Not content with the rather narrow limits of membership of E. Franklin Frazier's *Black Bourgeoisie* (Glencoe, Ill.: Free Press, 1957), nor with that book's emphasis on the mimetic character of black middle-class life

and on racial betrayal, Muraskin widens the scope of his study to include the large numbers of blacks whose income, even when earned in working-class occupations, allows them to publicly live the tenets of middle-class respectability; and he utilizes his well-researched and rich institutional case history to document their painful and contradictory structural position.

The key feature of the black middle class, in Muraskin's view, is its marginality. Its members are attracted by the cultural values of achievement and sobriety; they have internalized white expectations, yet are continually rebuffed by white society. In turn, they develop keen racial pride and animosities. But they remain cut off from the black masses. The black middle class may be black, but its members are more culturally alien from the black majority than from the white. For it is precisely differences in life-style that most clearly demarcate middle-class blacks from the mass. Suspended between black and white America, the black middle class has its own special dilemmas.

Prince Hall Freemasonry has been an institutional articulation of this position. It is not only an organization for blacks only, but it is an organization only for blacks who have demonstrated a commitment to middle-class mores. The dominant cultural motif is a quest for respectability. In big cities between 5% and 10% of black males over 21 are members; in small towns about one in three. But in both kinds of locales the same function is served: that of differentiating respectable from not respectable members of the black community. As in the case of the organization's first lodge, founded in 1776, membership is still selected on the basis of "real worth and personal merit only." Black masonry is thus an institutional replication of the doubly marginal position of the black middle class.

But Prince Hall Freemasonry is also a refuge from the tensions generated by the contradictory place its members occupy in the social structure. As Muraskin stresses, the Masonic order cushions the impact of middle-class marginality by creating and reinforcing a network which affirms the character of the members' life-style. Masonry socially and spatially separates its members from other blacks, protects them from "guilt by association," and provides an all-black setting for the performance of a wide variety of leadership roles of initiative and responsibility.

Within this world, more congenial than the society within which it is embedded, black Masons achieve much. They develop leadership abilities, provide the resources for significant self-help schemes, and provide the cadres for civil rights organizations. Black Masonry provides useful networks which make information about employment available, facilitate successful political activity, and fashion a strong sense of sharing and community among members. But these accomplishments, Muraskin notes, have come at the very high cost of a shattering estrangement from the majority of blacks. The organizational effectiveness of the Masons is measured by their ability to keep the boundary between the respectable middle class and the rest intact. Within the organization's bounds civility and trust may be created, but so is racial self-hate and a profound ambivalence about the character of fellow blacks. At the same time, this cost

is not offset by white acceptance. Masons accept the dominant articulated values of white society, yet are constrained by virtue of their color. The result is the reproduction of self-doubt, a compound of black middle-class images of the black mass and of exclusion from the bounty of white America.

All this Muraskin captures with remarkable sensibility, in part because his definition of the black middle class as that proportion of the black population that is publicly respectable accurately records the intrasubjective understandings of the group's members. Yet the definition's dependence on public propriety—"Those who act correctly are middle class, while those who violate the moral code in *public* situations are relegated to lower class status" (p. 13)—gives the historical analysis a static quality. What counts, finally, is visible activity, not market capacities or relations to the means of production. Having set aside these traditional Weberian and Marxist concerns, Muraskin's analysis either misses or downplays some of the most significant changes in the character of the black bourgeoisie.

Muraskin stresses that black Masons have from the start constituted a respectable elite, but he makes little of the vast shifts in the nature of elite recruitment. Before the Civil War, black Masons were drawn from the minority of free blacks, from Reconstruction to the turn of the century principally from the mulatto descendants of slaves. With the movement of large numbers of blacks to the cities of the North and Midwest after 1900, membership democratized to include those whose income from such jobs as rail porter, postman, janitor, and factory laborer allowed the choice of public respectability; the color complexion of black Masons darkened. Under the impact of the civil rights gains of the 1960s, there was a further change in the character of Masonic recruitment. Increasingly numbered among their ranks were government and private sector white-collar workers. Yet, significantly, Muraskin's data indicate that this improved labor-market position of the top third of the black population did not, as such gains had in the past, translate into equivalent gains in Prince Hall Freemasonry. Although Masons are still disproportionately drawn from the upper reaches of black society, "the Masonic Order has had difficulty in recent decades in attracting the college-educated black elite that once flocked to the fraternity" (p. 106).

Unfortunately, Muraskin leaves this shift virtually unexamined. One suspects that many members of this new black middle class, while still caught in the dilemmas so painfully etched in this volume, reject the terms of Masonic respectability. This group has individually and collectively been more successful in penetrating white society's occupational hierarchies and in maintaining a sense of racial solidarity. Many, though by no means all, have recognized that the elimination of the most direct and economically irrational discriminatory bars has still left the vast majority of blacks structurally situated within American capitalism in ways that are not resistant to the blandishments of self-help.

Race and Economics. By Thomas Sowell. New York: David McKay Co., 1975. Pp. ix+276. $9.95 (cloth); $4.95 (paper).

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Thomas Sowell is a brilliant purveyor of unfashionable economic ideas associated with the Chicago School. In his stimulating text, Sowell defines "race" in the broad sense found in everyday life—to designate ethnic groups of various sorts based on race, religion, or nationality. These groups are lumped into two camps dependent on the rapidity with which they have been economically successful. The most successful camp includes the Jews, West Indians, and Japanese Americans; the less successful are 19th-century immigrants (Irish and Italians) as well as 20th-century immigrants (Mexicans and Puerto Ricans).

Sowell puts great stress on the importance of rural or urban origins in explaining the rapidity with which various ethnic groups achieve economic success. According to Sowell, income is highly correlated with the amount of time an ethnic group has spent in the urban economy, either in the United States or in the foreign country from which they emigrated. Native blacks, who have been urbanized in large numbers for only two generations, are thus in many respects more comparable to such 20th-century immigrants as the Mexicans and Puerto Ricans.

The especially pernicious nature of U.S. slavery is also a factor which helps explain the poor record of native blacks relative to the successful West Indians. In Sowell's words, "Black immigrants to the United States have succeeded economically, educationally, and in other ways much more than native black Americans." In his discussion of the nature of slavery in the United States, he emphasizes the negative role played by our democratic ideals. Paradoxically, slavery has been less maligned in nondemocratic societies than in ones committed to democracy. As Adam Smith put it, "even in ancient times, slavery was more severe in democratic countries." Democratic processes would also seem to be more responsive to the best organized, more articulate, most prestigious, and best financed interests. As a result, those who already have more—particularly WASPs —tend to gain the most out of our democratic government.

Sowell sees no steady, one-way movement toward improved group relations. Rather there have been a number of detours, especially before World War I and during the Great Depression. In Sowell's words, "Although Roosevelt permanently captured the black vote for the Democrats and was responsible for a number of 'firsts' for Negroes, the period of the New Deal was not a period of black economic advance relative to whites" (p. 53).

There is a frank recognition of the fact that the greatest economic gains for blacks have occurred during our major wars, from World War I through the Vietnam War. Since the role of the federal government increased and the free market atrophied during all of these occasions, there

would seem to be some contradiction between these experiences and the Chicago School's basic thesis that it is the competitive market which reduces the possibilities for discrimination.

According to Sowell, regulated industries (as exemplified by the railroads) and governments in general not only have a poorer record on discrimination than unregulated industries but also reverse themselves more rapidly than unregulated industries once discrimination becomes a political issue. It seems clear that racial discrimination has become a political issue since World War II and that both the federal government and regulated industries have been in the vanguard when it comes to opening up opportunities for minorities.

Sowell finds no particular correlation between political and economic progress. The Irish and the American Negroes have enjoyed comparative success in politics, while the Jews and the Japanese Americans have been comparatively unrepresented in government. Instead, the latter prefer to concentrate on work, thrift, and education—more generally achievements involving planning and working for the future. The Irish and the Negroes, on the other hand, are supposedly more advanced in emotional and imaginative areas such as oratory, lyric literature, and music.

Is the black income distribution more unequally distributed than white income, as Sowell claims, echoing the conventional wisdom on this subject? Using the top 10% of the income distribution, he finds data to support his assertion, but as he indicates earlier, income from nonlabor sources is grossly underreported and is also concentrated among wealthy whites. If Gini coefficients are broken down on a regional basis, they are usually higher for whites than for blacks, thus indicating the opposite of Sowell's point and conventional findings.

Sowell naturally rejects such government actions as subsidies and affirmative action plans. Instead he advises blacks to place their faith in the free market and what has historically proved to be successful—attitudes of self-reliance, work skills, education, and business experience.

Handbook of Work, Organization and Society. Edited by Robert Dubin. Chicago: Rand McNally & Co., 1976 Pp. xi+1068. $25.00.

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This handbook is at once refreshing and disappointing, both of these judgments depending as much on what is not included as on the contributions themselves. What is refreshing is that the editor and his collaborators have avoided generating a litany of complaint of the kind that has too often characterized the recent literature on work. That complaint—sometimes cast in the language of alienation and sometimes not—has centered on the continuing inhumanity and exploitation in the workplace and on the assumption that alienated labor is at the root of a good deal else that

ails us in politics, family life, ethnic relations, education, leisure, and much more.

The complaint about contemporary work no doubt has substantial justification: there are a great many promising alternatives to be tried and old inequities to be challenged. But rhetoric, not analysis, has often dominated the discussion, along with presuppositions that are traditional but not well tested (e.g., the assumption that the job has the power to define one's identity and infect everything else). Thus it is refreshing to find that there is less panic and passion here than one might have expected.

But the dispassionate style is also, curiously enough, part of the disappointment as well. The intention was to produce a distinctive handbook whose uniqueness would lie in providing a guide to the future of work—not that futurology in the sense of specific predictions was to be practiced, but that the future "as a realistic leap forward from present conditions" (p. v) would be projected. It strikes me that, perhaps not too surprisingly given the range of topics and contributors, the editor has had his difficulties in realizing this intention. For the most part what emerges is a fairly standard and very competent set of reviews of varied domains relating to work.

These domains are covered in 23 chapters, loosely (i.e., not very compellingly) organized into eight parts. Thus, Part VIII (titled "Postindustrialism") is composed of a casual introduction and one chapter (by Eric Trist) which makes a case for the emergence of new individual values (e.g., the emphasis on self-actualization and self-expression rather than achievement and self-control), as well as new organizational philosophies and strategies in the postindustrial era. This is not a warmed-over "greening of America"; it is much better and more provocative than that, but it cannot alone constitute an adequate treatment of the postindustrial idea.

There is also a series of chapters on work experience in other countries: in Japan (Bernard Karsh), Poland (Alexander Matejko), Israel (Theodore D. Weinshall), Yugoslavia (Janez Jerovsek), and Norway (Einar Thorsrud et al.). The latter is particularly helpful since a comprehensive review of the Norwegian innovations has not hitherto been available in English. The section of "work in different social systems" also includes a chapter by Henry A. Landsberger, whose purpose is to apply the concepts associated with the field of stratification and social mobility to labor and peasant movements, with considerable emphasis on the notion of collective group mobility as contrasted with individual mobility.

The section on "Work and Society" (Part VI) includes two papers, one on "Occupational Power" by William H. Form and Joan A. Huber and another on "Work and Political Behavior" by Erik Allardt. The former features a firm analysis which stresses the effects and processes of power rather than its agents (i.e., it shows less interest in powerful persons and more in power outcomes, namely, increased income and control over conditions of work). Allardt canvasses the well-known literature on work and politics (e.g., Blauner, Dubin, Etzioni, Goldthorpe, Lipset, Mills, etc.)

and features the disheartening but correct summation that "it is very difficult to find conclusive results in regard to the political effects of working conditions" (p. 817).

Unfortunately, but predictably, as any literature summarizer eventually discovers, such inconclusiveness bedevils the effort at many points. In one way or another the common line reads, "We know some but not too much about ———." One can fill in the blank with "organization development" (see George Strauss's chapter by that title, dealing with various forms of management training); "decision processes" (Frank Heller); "comparison processes" involved in workers' attitudes about compensation for work (D. W. Belcher and T. J. Atchison); "shop floor behavior" (Thomas Lupton); "the language of work" (Martin Meissner); or even "attachments to work," which is Dubin's central concern in a chapter providing a comprehensive perspective about that issue and some cross-national data on it. In some of these instances (e.g., Heller and Meissner) the difficulty in generalizing leads to a heavy concentration on the author's own recent work in the area, and rightly so, I think, although it does leave some questions about the "handbook" format.

The section on "Work Organizations" (Part IV) features what amounts to a debate regarding job characteristics and worker motivation. One side of the debate contends that the emerging postindustrial era involves the development of a sociotechnical system which demands and produces new worker needs (e.g., autonomy), skills (e.g., monitoring and diagnostic operations), and relationships (e.g., the redefinition of worker "supervision"), and that these are now finding their expression in successful job redesign programs that have consequences both at the plant and away from it (e.g., increased community participation by the worker). That view is cogently and carefully argued by Louis E. Davis and James C. Taylor in their chapter on "Technology, Organization and Job Structure," and to some extent as well by Curt Tausky and E. Lauck Parke in their review of "Job Enrichment, Need Theory and Reinforcement Theory." The other side of the debate, the skeptical side with respect to the theories emphasizing self-fulfillment that have come to be identified with Maslow, McGregor, and Herzberg, is expressed in Mitchell Fein's review of "Motivation for Work." As a professional engineer, he argues with some force, and conservative elan, I think, that the Lincoln Electric Company's thoroughgoing and successful profit-sharing plan is a better design for motivating workers than the work-fulfillment models so favored by social psychologists and job redesigners.

To round out the contents, the opening chapters consist of an overview by Robert Dubin on "Work in Modern Society" and a review of "Work and Leisure" by Stanley R. Parker and Michael A. Smith; Part II ("Work and the Individual") consists of two papers as well: one on socialization to work (John Van Maanen) and "work and careers" (John O. Crites). Finally, a chapter on "Schools as Work Settings" (by Roland J. Pellegrin) is included in Part III, which carries the heading "Working Behavior."

This collection is, inevitably, strong in some respects and weak in others.

The themes that appear and reappear across these chapters warrant the judgment that it is strong in its treatment of two aspects of work alienation: the theme of autonomy, control, and participation in the workplace, and the theme of fulfillment, engagement, and creativity in work. It is also strong where it sets out to be, namely, in the treatment of work as an activity which is embedded in a larger context of technical, organizational, and societal forces. It is relatively weak in its treatment of work in western Europe and in the developing countries and in its treatment of the strategic-political involvements of work (e.g., unions and industrial conflict).

On the whole, this work succeeds in shaping and summarizing a massive literature on work, and some of the disappointment no doubt derives from the book review process itself. One does not go to a 1,000-page handbook for refreshment (Studs Terkel's *Working* [New York: Pantheon, 1974], Sennett and Cobb's *The Hidden Injuries of Class* [New York: Vintage, 1972], or Aronowitz's *False Promises* [New York: McGraw-Hill, 1973] are all considerably more engaging), and one probably ought not go to such a handbook with the task and intention of reading it through for a review and a deadline. However that may be, I come away with a conviction that I believe many of the book's contributors share—namely, that if work is the centerpiece of existence, as some of our theories propose, we have not done very well as yet in understanding either its daily character or its consequences.

On the Nature of Organizations. By Peter M. Blau. New York: John Wiley & Sons, 1974. Pp. ix+358. $14.50.

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There used to be a clever television commercial stressing the theme that when E. F. Hutton talked, everyone listened. Peter Blau is probably the most distinguished American theorist in the organizations field, and therefore when he publishes a book all of us turn our heads and take notice. This book represents a collection of Blau's work spanning approximately two decades of creative contributions to the organization literature. All of the papers were published previously, the earliest in 1954 and the most recent in 1973, and all are well worth reading and rereading today. They are models of clarity, thoughtfulness, and commitment to the rigorous development of social theory.

The book has a very helpful general introduction and is divided into three sections, each with an excellent set of prefatory comments that describes the themes underlying the sets of papers and their relationship to the author's current thinking. The essays as a whole reveal the author's remarkable theoretical range, perspicacity, and ability to study important problems programmatically. They also reveal an admirable willingness to

change methods and approach as a product of direct experience. Unlike some scholars, he admits past errors in interpretation and maintains a healthy respect for data and for alternative views.

Part I, entitled "Approaches, Concepts, Methods," consists of six papers which present an analysis of Weber's theory of bureaucracy (Max Weber is listed in the book's index more than anyone else—16 times), a classification of dimensions of work organizations, a description of the structural effects method, a discussion of problems encountered when doing field work in organizational settings, and an essay on the nature of comparative analysis. In the sixth paper Blau emphasizes the need to distinguish three levels of analysis: individuals in organizations, groups of interrelated persons and social processes in these groups, and the characteristics of organized systems as a whole. Over and over again Blau makes clear the need for a large sample of comparable organizations to test hypotheses on the latter level, an approach he is convinced is most essential if genuine progress is to be made in learning about how organizations function.

Part II, entitled "Informal Processes in Work Groups," also consists of six papers. Four of these deal with case studies of competition and cooperation in a work group, the effects of statistical records of performance, patterns of consultation among colleagues, and the consequences of bureaucratic procedures and informal relationships among case workers on orientations toward clients The last two papers focus on theoretical development and consist of his theory of social integration and a partial explication of the version of exchange theory presented in greater detail in the author's book on *Exchange in Power and Social Life* (New York· John Wiley & Sons, 1964).

The final section concerns "The Formal Structure of Organizations." Here Blau replaces the emphasis on case-study analysis with a set of papers dealing with what he now considers *the* critical problem of organizations: ". . . what the interrelations between various structural attributes of formal organizations are." If you share Blau's conviction that progress will be made primarily at this level, you will be most pleased with the papers in this section. The findings reported in chapter 15 that show, for example, that research in academic institutions is more highly valued than teaching, and that having a set of colleagues committed to research creates an environment that promotes research involvement, are not likely to generate a great deal of research interest. The crowning achievement of this work is the formulation of a rigorous deductive theory of organizational differentiation that seeks to explain (in Braithwaite's sense of the term "explain") the relationship between organizational size and differentiation. This is one of Blau's favorite papers and is, as physicists are prone to say, the most "elegant" of them all.

The author is very much taken with paradoxes or dilemmas, and so I conclude with one for him. Blau is a person with an extraordinarily keen intellect, someone much aware of the increasing importance of large organizations in society. In *The Structure of Organizations* (with Richard A. Schoenherr [New York: John Wiley & Sons, 1971]) and in the present

book, deep concern is expressed over the fact that "the concentration of organizational power in the hands of a few men shielded from public surveillance and control poses a serious threat to democracy" (p. 20). The same theme is repeated in the last and most recent paper of this volume, where he states, "A small handful of persons commanding large resources and empowered to formulate basic policies and to make the basic organizing decisions is able to direct the work of hundreds of thousands of employees and to exercise tremendous influence in the society" (p. 342). Does Blau believe that by revealing basic knowledge on how organizations function he will eventually contribute to a greater understanding of this problem? Four features of Blau's research struck me in this regard as especially significant: (1) the work reported dealt exclusively with American work organizations, (2) the organizations studied—government bureaus, department stores, hospitals, colleges, and universities—were predominantly middle-class, professionally dominated social systems; (3) in most cases the highest levels of authority were not assigned special attention; and (4) the data collected tended to be of the one-shot ahistorical variety. The paradox is how can one hope to understand the concentration of organizational power today and ignore worldwide influences (especially those stemming from the economic sector, that is, the multinational corporations) which are inducing massive occupational and organizational changes; not consider the problems of organizations, such as jails, prisons, the enlisted military, and mental institutions, which contain significant portions of the society's underclasses; not consider the highest levels of government organization which are evidently of increasing significance; and, finally, neglect historical trends in the development of work organization. No doubt the criticism smacks of unfairness, for who is to say in the end which approach will bear the most fruit? Whether or not Blau attacks the problem of the concentration of organizational power in his current fashion by seeking to develop broad explanatory formal principles or selects a macrocomparative-historical perspective, all of us will continue to listen and profit immensely from the pen of this sagacious scholar. As Ulysses said, "Brothers [and sisters] ye were not formed to live like brutes, but to follow excellence and knowledge."

A Sociology of Organizations. By J. E. T. Eldridge and A. D. Crombie. London: George Allen & Unwin, 1974. Pp. 218. £4.25 in U.K. only.

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J. E. T. Eldridge and A. D. Crombie have provided a brief, thoughtful, and well-written introduction to many of the major ideas, problems, and issues in the sociological study of formal organizations. The first three of the book's five parts deal with the definition, labeling, and classification

of organizations and include two long chapters on modes of analyzing organizational structures and processes. In these two chapters the authors put forth a conception of organizational structures as the products of interplay between environmental constraints and "strategic choices" by organizational leaders. They do a particularly good job in these chapters, which are written around the ideas of organizational missions and cultures, of integrating systems theory with structural theory, and of balancing an emphasis upon the uniformity of organizations having similar missions with a counteremphasis upon organizational individuality (cultural distinctiveness). In part 4 they go on to place organizations in wider social and political contexts. In doing so they range quite a bit further than most introductions by giving short but incisive treatments not only of Weber but also of Spencer, Durkheim, and Marx (et al.) and then contrasting totalitarian, oligarchic, and pluralistic models as developed or exemplified in the works of R. Aron, C. Friedrich and Z. Brzezinski, W. Kornhauser, R. Lane, R. Dahrendorf, F. Neumann, H. Arendt, R. Michels, S. M. Lipset, L. Gulick and L. F. Urwick, P S. Florence, T. Burns, A. Gouldner, and C. W. Mills and J. Schumpeter, to name a few. In the final part they examine political and ethical issues, looking first at criticisms of sociologists as organization men and then their (our) potential to serve as agents of change. They conclude: "The sociologist as change agent does not have to operate with a model of man in organization as puppet or of organization structures as immutable. A realistic analysis of the possibilities for change in a society of organizations has to take into account the resources and intentions of the powerful. Such restraints are, however, the penultimate rather than the last word on the matter. They are a gloss on Marx's contention that men make their own history albeit in circumstances not of their own choosing" (p 204). These final chapters alone would make the book a useful supplement to most organizational texts that are currently available.

The title, *A Sociology of Organizations*, apparently expresses the authors' conviction that ". . an economics, or a psychology, or, in our case, a sociology of organizations has a claim to be considered in its own terms, rather than as some inferior species of activity the value of which is only to be measured in terms of its contribution to general organization theory" (p. 14). Few readers (at least among the subscribers to this journal) would be expected to disagree out of hand. The question is, Does the book help the reader to evaluate the claim? The answer is only a qualified yes.

The qualifications are two. First, while Eldridge and Crombie present the field in an appropriately sociological fashion, principally stressing the organizational and societal levels of analysis, they do not consider in any detailed or systematic way the empirical research upon which the claim must ultimately be judged. Books of theory without data are obviously no crime. But is it fair (or wise) to invite judgment on the basis of programs and abstractions alone? Second, Eldridge and Crombie begin the book by observing that there is not now a single unified sociology of

organizations. Commenting upon the ad hoc quality of most collections of readings they write: "This should give us cause for reflection. The problem one confronts is that separating off sociological studies from other studies of organizations, insofar as that is possible, does not reveal a particular kind of conceptual unity (although some sociological imperialists are not above trying to impose it). What we discover is that sociologists study organizations (variously defined) for different thematic and theoretical reasons" (p. 14). They go on to identify these different reasons in terms of Barney Glaser and Anselm Strauss's distinction between formal and substantive theories and of David Silverman's distinction between social system and social action approaches. To write "a sociology of organizations" in the face of wide and perhaps irreducible variation among sociologists, reasonable next steps might be to present the full array of problems and viewpoints in a systematic, comparative manner and to assess the extent to which they derive from and are pertinent to a common core of uniquely sociological concerns. Eldridge and Crombie do neither fully. They inform the reader of the diversity and show him some common themes, but they leave him wondering at the end almost as much as he wondered at the start whether the variation among sociological studies of organizations is really enough less than or enough different from the variation between these studies and those done in other disciplines to warrant the book's title.

The Growth of Bureaucratic Medicine: An Inquiry into the Dynamics of Patient Behavior and the Organization of Medical Care. By David Mechanic. New York: John Wiley & Sons, 1976. Pp. xii+345. $14.95.

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"In sum, sociological contributions to understanding the medical care sector are highly limited. Much of the problem involves the failure to specify the aspects of the health care sector of greatest significance to sociology, in contrast to health economics or health administration. Moreover, few sociologists have directed themselves to the role of medicine relative to other social institutions and to the larger social and cultural significance of health care institutions" (p. 31). This book represents the author's response to his own challenge. In brief, David Mechanic sees the relationship between providers and patients of primary significance to sociologists, particularly as this relationship is influenced by organizational settings and their larger social, economic, and cultural determinants.

A common theme throughout the book is the need for organizational structures and relationships which will reconcile the growing demands of new medical knowledge and technology with the common needs and expectations which patients bring to physicians. Ironically, it is often the public's expectations and demands for the latest advances in medical

knowledge and technology which create conflict with competing wishes and desires—a phenomenon the author tends to gloss over.

Further, while a major portion of the book is devoted to a critique of the relative failure of current medical care organizations to both incorporate the latest advances in medical technology and provide personalized care, little attention is given to the fact that there are individuals who are simply inconsiderate or "poor" patients. That not all problems associated with medical care delivery are due to providers or the systems in which they work but, rather, may be due partly to inconsiderate and unrealistic demands or expectations needs to be considered and incorporated into future analyses.

The above notwithstanding, this is an important book, extremely well written with a propitious blend of theory and evidence. It is divided into four parts. Part 1 traces the growth of bureaucratic medicine in western societies; part 2 describes the relationship between patient behavior and the design of medical care organizations, part 3 is devoted to the empirical study and implications of this relationship; and part 4 provides a broad orientation to issues of health policy, social regulation, and evaluation of health services.

The first three sections are well integrated; the fourth seems to have been added as an afterthought While containing several important insights, it could have been greatly shortened or deleted altogether without destroying the book's basic integrity.

In part 1 fundamental differences in the perspectives of patient and physician are described. Patients seek care for the relief of symptoms that are almost always discomforting and, at times, frightening, while physicians think in terms of diagnostic categories and treatment regimens for applying their techniques. This is complicated by the growing bureaucratization of medicine and an expanding set of providers which tends to divide and diffuse the responsibility for patient care. This phenomenon is not unique to the United States but may be observed among most technologically developed countries.

The author notes that a major problem in the comparative study of medical care organization is the inadequacy of traditional concepts in dealing with new structural arrangements and ongoing organizational processes. Mechanic develops his own framework based on a given country's ideology, economic organization, level and character of technology, professional organization and dominance, organization of the health professions, access to medical care, distribution and organization of health services, and quality of care (p. 29). While useful as a gross categorization, much work remains to be done before the framework can become empirically useful. In particular, the empiricist will be disappointed that the author did not push ahead and develop operational indicators for his concepts.

The most important section of part 1 is chapter 4, in which the implications of organization theory are discussed. The author notes the lack of management and organizational know-how to deal with competing

objectives. This is particularly true in regard to professional recruitment and socialization, incentive structures, communication and information networks, and the design of work rules and specifications. Herbert Simon's notion of bounded rationality (*Administrative Behavior,* 2d ed. [New York: Free Press, 1957]) is contrasted with Chris Argyris's normative theory of self-actualization (*The Applicability of Organizational Sociology* [Cambridge: Cambridge University Press, 1972]).

Mechanic states that neither view adequately defines the individual's needs within organizations. Instead, he argues persuasively that such considerations are the result of continual interaction between individuals and their work. Individuals simultaneously shape, and are shaped by, organization structure. For example, physicians will structure their work differently depending on the payment system involved. Considered in this light, the author argues that individual member autonomy may be less important than a sense of commitment to valued goals, the means to solve problems effectively, and supportive relationships.

It is in the consideration of these issues that health care organizations may find the means to adapt quickly and efficiently to new environmental demands while maintaining personalized care and integrity of service. The author's arguments could have been further strengthened by a fuller consideration of the more recent contingency theory literature which provides a number of specific suggestions for the design of health care delivery organizations.

Part 2 builds on the general framework by elaborating on the nature of patient-physician relationships in different practice settings. The influence of practice structure on physician behavior is analyzed. Drawing on his own work in the United States and abroad, Mechanic finds a positive relationship between the number of patients seen and the doctor's tendency to describe patient complaints as trivial or inappropriate. From such empirical work, Mechanic develops a tentative model of the factors affecting physician behavior, including specialty, attitudes toward the work ethic, social orientation, patient mix, patient demand, perceptions of triviality, degree of bureaucratic organization of practice setting, satisfaction, and related variables.

Chapter 7, by Tessler and Mechanic, is concerned with consumer behavior in a prepaid group practice versus a fee-for-service system. A strong relationship is found between perceived access to care and patient satisfaction, with generally less satisfaction experienced by patients in the prepaid group practice setting.

Part 3 begins with a general discussion (provided in chap. 8) of the processes by which patients and physicians attribute meaning to various signs and symptoms. Too often the intentions and motivations of physicians fail to match those of patients. This is a particularly crucial issue in regard to patient compliance with physician advice. The management of information is identified as a key element in the doctor-patient relationship, with both too little and too much information being potentially harmful to the patient and restricting the physician's ability to help.

Chapters 9–12 are empirical studies of patient-physician interaction conducted by the author's colleagues and former students. Chapter 9, by Greenley and Mechanic, focuses on the help-seeking behavior of university students faced with psychological problems. Their findings indicate that sociocultural variables directly affect help-seeking behavior, independent of any effects exerted indirectly by producing the distress or illness itself.

Chapter 10, by Doris Slesinger, describes a study of the utilization of preventive services by urban black mothers. Testing a sociocultural model of preventive medical behavior, she finds that socioeconomic status and social setting variables explain most of the variation in use of preventive services. Most significantly, those children who had used fewer preventive services were also in poorer health.

Chapter 11, by Bonnie Svarstad, focuses on the relationship between physician-patient communication and patient compliance with medical advice. Compliance was found to be higher where there was an explicit attempt on the part of the physician to both instruct and motivate the patient and where there were fewer complaints from the patient regarding the treatment plan.

Chapter 12, by Linda Aiken, focuses on the social-psychological adjustment of patients following acute myocardial infarction. Particularly striking is the apparent lack of information on the part of those closest to the patient (spouse and children) regarding the illness, the immediate recovery process, and the role of the spouse and child in the long-term rehabilitation.

As indicated, part 4 is concerned with large-scale trends in health policy and is less integrated with previous sections of the book. The issue of informed consent in experimentation is discussed in chapter 13. The issue is treated in a well-balanced fashion, as indicated by Mechanic's statement that "society cannot realistically impose new obligations on researchers without providing the *means for their implementation* or making clear what *other priorities are to be sacrificed*" (p. 263; my italics).

In regard to the relationship between social policy and health policy, the author suggests that the largest impact of social science is not on public policy directly but rather in shaping the general climate of opinion in society. In particular, the author believes it is important for social scientists to develop perspectives and conduct inquiries on problems which are not immediately evident to the policymaker or practitioner.

Since there is great emphasis at present on "immediate turnaround" policy-relevant research and analysis, the author's views are particularly noteworthy. In my opinion, without investigation of nonevident problems and continuing theoretical and methodological work in the social science disciplines, health services research and policy analysis becomes, in the long run, both intellectually bankrupt and pragmatically useless. A balance is suggested between immediately useful social science research which can be readily translated into policy alternatives and actions and research which tackles more complex problems and produces results likely to be useful only in the long run. In either case, but particularly for short-run

results, Mechanic notes that social scientists need to be in closer proximity to decision makers involved in the policymaking process.

In the final chapter the author states his preference for a form of national health insurance that covers the entire population, provides access to medical care without financial barriers, allows for experimentation in delivery methods, and has its economic control and accountability vested in a central governmental source. The analysis is brought down to the level of the doctor-patient relationship with emphasis on the need to take into account the desires of *both* patients and doctors. Particular consideration needs to be given to the effects of various proposals on physician practice habits. Mechanic believes that it is in these areas that major costs are incurred, not at the patient's point of entry into the system.

The reader who is looking for detailed specifications or solutions to problems will be disappointed. Mechanic notes early in the book that no blueprints are to be offered. Rather, he and his colleagues attempt to provide greater understanding of the complexities of medical care delivery and the actors and institutions involved in the daily transactions. As such, the book is clearly relevant to medical sociologists and others interested in the investigation of health services issues. In fact, an important contribution of this book is the large number of issues raised for further research, for example:

1. How does the approach to professional regulation in different countries affect physicians' work and the extent to which their power and responsibilities exceed their usual medical work?

2. What particular forms of prepaid group practice are conducive to greater patient satisfaction, more efficient operation, and higher quality of care? There is a need for large-scale, systematic comparative studies of prepaid group practices.

3. To what extent does the organizational structure account for differences in patient care outcomes in prepaid group practice versus elective group practice?

4. What organizational mechanisms of coordination and conflict-resolution exist to facilitate the work of the organization while still protecting patient rights?

5. In what ways do organizational settings reinforce or fail to reinforce new provider roles (physicians' assistants, nurse practitioners, etc.)?

6. In what ways do patient mix and the range of primary care functions affect physicians' perceptions and behavior concerning diagnosis, treatment, and patient education?

7. What factors are most related to the ability of the physician to secure the patient's cooperation in following medical advice?

8. To what extent is it possible to develop clearer causal models of help-seeking behavior and health services utilization?

9. What are the consequences of disclosure of information to the patient? How much information should be disclosed, of what sort, in what form, under what circumstances, and with what risk?

Answers to these and related questions have the potential to both

advance the understanding of medical care delivery and contribute to an increasing body of knowledge and theory in the social sciences. This book is important as a starting point for addressing many of these issues.

Minority Men in a Majority Setting: Middle-Level Francophones in the Canadian Public Service. By Christopher Beattie. Toronto: McClelland & Stewart, 1975. Pp. xi+224. $4.95 (paper).

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Beattie's interesting book focuses on French-English differences and French-English relations at the middle level of the Canadian public service. A national bureaucracy is an integrative force in a society divided into ethnic subsocieties. The study examines this "interplay between forces of integration and separation" (p. 1). It is organized with a straightforward conceptual vocabulary borrowed from H. L. Wilensky and J. Ladinsky's work on Catholic and Jewish professionals in Protestant-dominated organizations in the United States. Anglophones (*majority men*) are dominant in the public service (a *majority setting*). The Francophones are *minority men.*

Interviews were conducted with 168 Anglophone and 128 Francophone middle-level public servants working in five government departments in the national capital area (Ottawa-Hull). The data were gathered under the sponsorship of the Royal Commission on Bilingualism and Biculturalism in the mid-1960s. The sample was designed to include those men who, although still young (between 25 and 45), had several years of their work life behind them, were at senior levels (but not at the top), and expected the future to be one of advancement.

The Francophones are clearly at a disadvantage. Persons of French descent make up 18% of the middle-level sample, as against 28% of the Canadian population. Among the Francophones, Quebecois are underrepresented; French Canadians from outside of Quebec are overrepresented. One reason the Quebecois are underrepresented is that the "cultural character of the Public Service or the political ends it serves, or both, do not appeal to them" (p. 51). Francophones from Quebec prefer to pursue careers in Quebec, where there is a full range of French-language educational and cultural institutions. Moving to Ottawa often means working in English and living in an area with far fewer French-language cultural institutions. The Quebecois who *are* recruited to Ottawa come from a wide range of class backgrounds, much like the Anglophones in the sample, and have higher educational credentials (p. 31). The Franco-Ontarians, most of whom are from the Ottawa area, are a disadvantaged group which has long had problems obtaining adequate French-language schooling. This has led to a high dropout rate and a generally lower education level over time. They account for half the middle-level

Francophones, are less likely to have university degrees, are more fluent in English, and are concentrated in routine work settings. The Francophones "were considerably more likely than the Anglophones to consider abandoning a career in the federal administration soon after it began" (p. 82). More of them indeed *do* quit. Beattie notes that "the wisdom of making only a tentative commitment is often verified when they discover the language and career disadvantages they have to overcome" (p. 83).

For equally large segments of both language groups the federal administration is an important avenue of upward mobility. With respect to career mobility, regression analyses of salaries are presented, some of which have been reported previously in this *Journal* (*AJS*, 1971). "When the educational levels and career types are divided into age groups, it is apparent that young minority men are doing as well or better than their majority counterparts. It is in the older age groups that Francophones are at a disadvantage" (p. 168). Why is it that the disparity between the two groups grows over time? Several explanations are considered. Beattie concludes that the main reason is the subtle, informal fact of career discrimination: many Francophones are excluded from the informal learning experiences, socializing, and encouragement by superiors that are crucial for advancement. It should be noted here that discrimination is conceptualized as a residual category to which explanatory power is attributed after other independent variables are taken into account. The discussion is insightful, but no evidence is presented about the content of the residual category. The absence of such evidence illustrates the difficulties of studying mechanisms of domination in organizations. (For related findings in a similarly designed study, see John P. Fernandez, *Black Managers in White Corporations* [New York: Wiley, 1975].) Beattie does point out that a structural root of this discrimination is that those who have trouble with and feel less at ease in English cannot participate as fully in formal and informal work relations. Whatever we call it, if groups A and B share an organization, one very handy mechanism of domination is for group A to impose its language on group B as the language of work.

In this connection, it would have been interesting to get contextual information about the language of work and the proportion of Francophones in each man's work unit. Whether Francophones use French at work would seem to depend more on their level than on what department they work in: the higher their salary, the more they work in English (p. 149). An interesting finding is that creative work settings such as research and policymaking are more conducive to Francophones' "assimilation" (measured by whom they name as their best friends) than settings which involve more routine work. I found myself wondering whether this was due to the difference between the types of work done, or whether the Quebecois in creative settings simply had fewer Francophone colleagues and were further from family and friends in Montreal than the local Franco-Ontarians working in routine settings alongside their brethren.

In the choice of a comparative perspective, Beattie follows in the footsteps of Lipset, Alford, and others who have compared the Anglo-American democracies. Some tentative and interesting hypotheses are advanced comparing the fate of white minority men in majority settings in the public bureaucracies of Canada, Australia, the United States, and Britain. Although this tradition has produced much of value in the study of the class structures and political cultures of the four countries, I suspect it is not the best comparative perspective to adopt when the aspect of Canadian society one is focusing on is French-English relations in eastern Canada. The existence of two territorially based, highly segmented *linguistic* societies (each with its own set of institutions) in eastern Canada is precisely what makes Canada most unlike the other Anglo-American countries. This study should serve as an invaluable basis for researchers planning future comparisons with the civil services of Belgium, Switzerland, and other countries where more than one language is used.

For many decades Francophones have been underrepresented in the federal public service, and the language of work has always been mainly English. This was legitimated by the notion that a unilingual English administration was more "efficient." In recent years government efforts have centered on two related goals. The first of these, similar to affirmative action programs in the United States, has been to increase the proportion and position of Francophones in the public service. The second goal has been to equalize the status of the two languages, to make a "career in French" as possible as a "career in English" has always been. To find out their effect, Beattie did a 1973 poststudy of his original 1965 sample and found that although "there has been a slight reduction in discriminatory practices" (p. 192), "the Anglophone-Francophone salary gap for these middle-level men has not narrowed" (p. 188).

The appearance of Beattie's book is important in this context. It will be of interest to students of comparative intergroup relations, organizations, and occupations, and especially to those attempting the needed theoretical linkages among these areas.

Canadian Political Parties: Origin, Character, Impact. By Frederick C. Engelmann and Mildred A. Schwartz. Scarborough: Prentice-Hall, Inc , 1975. Pp. ix+358. $6.95.

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In this volume Frederick Engelmann and Mildred Schwartz provide a comprehensive treatment of the Canadian party system which is based in part on their earlier book, *Political Parties and the Canadian Social Structure* (Scarborough: Prentice-Hall, 1967). The framework for this study is a three-dimensional typology of political parties. Parties may be organized on a mass or cadre basis, they may have broad or restricted

support among the citizens, and they may make pragmatic or principled appeals to the electorate. The Canadian federal party system has been characterized by the dominance of two parties, the Liberals and the Conservatives, which are organized on a cadre basis and make pragmatic appeals to the electorate. Although both parties had broad support during the first 50 years following confederation, Conservative support has tended to be restricted to the non-French population since the conscription crisis of World War I, and the Liberals have frequently failed to acquire western support since the emergence of third parties there.

Given this party typology, Engelmann and Schwartz develop a systems analysis of the Canadian political parties by examining relevant inputs and outputs. Major inputs are generated by basic societal cleavages, the governmental system, and special influences, which include elites, the mass media, and organized interest groups. Central outputs include the provisions for leadership, the making of decisions, and the mobilization of the electorate. The input-output analysis is supplemented by an extensive discussion of party organization, support, and programs.

The analysis of the relevant governmental and societal inputs is impressive, despite an excess of systems terminology in the discussion of societal factors. The implications of federalism and of the electoral and parliamentary systems are clearly spelled out. Societal inputs tend to revolve around basic cleavages associated with nation building and the industrial revolution. In this regard, it is not clear that one gains any analytic leverage by subsuming oppositions between primary and secondary sectors of the economy and between workers and employers under the "functional-economic dimension" of cleavage (p. 71), or church-government conflicts and regional differences under the "territorial" dimension (p. 71). Their substantive analysis of these problems, which is very thorough, does not really benefit from such language.

The most problematic section of the study is probably the one dealing with special influences. Although the authors consider the impact of various elites on Canadian political parties, they do not give sufficient attention to the possibilities that some elites, particularly the economic elite, exercise more influence than others; that elite interaction may figure in political activity; or that elite members may possess common social characteristics that facilitate similar outlooks. The ways in which Engelmann and Schwartz address the question of elite structure lead them to discount John Porter's seminal work on these topics, *The Vertical Mosaic* (Toronto: University of Toronto Press, 1965). The query should not be whether Canada has a "monolithic power elite" (p. 115 [Porter himself is quite a strong advocate of the plural elite position]), or whether members of the political elite are engaged in "any form of conspiracy, deliberate or otherwise" (p. 117). Rather, the task is to demonstrate how configurations of power and social class set parameters for political activity.

One encounters similar difficulties with respect to Engelmann and Schwartz's treatment of the mass media and organized interest groups.

They provide an excellent analysis of the types of partisanship exhibited in the media and demonstrate the influence the media exert on political style, but they do not connect media activities to the general elite structure. Given Porter's findings regarding the links between the mass media and other elites, as well as the more detailed work done by one of his students, Wallace Clement (*The Canadian Corporate Elite* [Toronto: McClelland & Stewart, 1975], which postdates the volume under review), it seems clear that ties between these elites may be important conditioning factors in the operation of Canadian political parties. In a parallel fashion, the discussion of organized interest groups fails to distinguish differences in the amount of power or influence exercised by such groups. Although we are presented with a detailed account of their structure and operation, there is no clear indication that some interest groups have institutional and elite affiliations which make it more likely that their demands, as opposed to others, will be met.

The most innovative part of the book is the concluding chapter. It is less a conclusion than an attempt to relate the party typology discussed above to competitive structures in order to generate hypotheses about the relationships between party and society. According to Engelmann and Schwartz, a party system is competitive when each of two parties receives at least one-third of the total vote cast. A given party is dominant if it polls more than one-third of the vote, while others receive less than one-third. In accordance with such distinctions, Canadian federal and provincial elections are then classified in terms of major patterns, the former from 1867 to 1972, the latter from 1919 to early 1974. The major finding is that elections at both levels have been competitive affairs among cadre-pragmatic parties but that parties with some principle-based appeal have been frequent participants in provincial elections.

Engelmann and Schwartz then elaborate a set of 15 hypotheses which relate parties to society in terms of leadership, policies, resource mobilization, effectiveness, and legitimacy. Some of the more interesting ideas include the following propositions: where "cadre-restricted-pragmatic parties compete, the result will tend to be unstable minority government" (p. 321); and where "dominance is accompanied by fragmented social bases, largely because cross-cutting loyalties are minimized, there will be greatest difficulty in responding to crises of legitimacy" (p. 323). The former hypothesis is substantiated by the fact that the federal elections of 1925, 1962, 1963, and 1972, which involved two pragmatic parties with restricted social bases, resulted in short-lived minority governments. The latter proposition is supported by an analysis of the events surrounding the conscription crises of the two world wars and the crisis of October 1970.

Some of the other strengths of the book deserve mention. The section on the character of Canadian political parties goes into much greater depth than the authors' first volume on questions of party support and policy formation. With regard to party support, Engelmann and Schwartz develop the concepts of electoral mobilization and party polarization and

use them to good advantage. The entire work is strengthened by the authors' firm command of Canadian political history; they continually elaborate theoretical points with historical references. Furthermore, the joint efforts of a political scientist (Engelmann) and a sociologist (Schwartz) produce a clear understanding of the relationship between party and society which does justice to both political and social concerns.

This book, like their previous one, is likely to be the standard source on political parties for another generation of students of Canadian social science. It goes well beyond its predecessor in terms of the incorporation of new findings into previously discussed topics, the areas covered, and the overall integration of this material into a more refined systems model. In addition, the theoretical structure and the related attempt to establish testable hypotheses about the operations of the Canadian party system establish its relevance for current research in the area.

White-Collar Power: Changing Patterns of Interest Group Behavior in Sweden. By Christopher Wheeler. Urbana: University of Illinois Press, 1975. Pp. xii+210. $12.00.

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The aim of Christopher Wheeler's book is to give a picture of interest group politics in Sweden in the 1950s and 1960s. This is done through an analysis of the political activities of the Central Organization of Salaried Employees or the TCO, which is the biggest white-collar organization in Sweden, embracing salaried employees in the lower and middle income brackets within the private as well as the public sector.

Wheeler studies the impact of the TCO's policy-influencing tools—such as membership on government commissions and administrative agency boards and the so-called *remiss* replies to commission proposals—within three issue areas: supplementary pensions, inventors' rights, and school reform. The three cases are representative examples of different degrees of politicization: the supplementary pensions reform in 1959 was the greatest and most intensely contested social reform issue in Swedish history, whereas inventors' rights was a technical question with direct relevance only for small groups of salaried employees, and the school reform issue, which dealt with the new comprehensive school curriculum in 1969, held an intermediate position with respect to political effect. Wheeler concludes that the TCO's influence over public policy is greatest when political partisanship is least, and to the extent that the leadership succeeds in keeping issues from being politicized—in gaining access to the early stages of the decisionmaking process—its influence is often decisive.

The inventors' rights and school reform issues are good examples of areas in which the TCO, acting through administrative channels rather than through the political parties, takes the initiative early and applies

decisive pressures. The most interesting part of Wheeler's book is the chapter in which he shows how the TCO takes the lead in a curriculum reform through its delegate to the National Board of Education and builds a coalition there with the Confederation of Swedish Trade Organizations or the LO, without interference from the opposition parties. He is quite right in saying that the position of some administrative agencies —with the modern and typically Swedish type of so-called laymen boards —has been strengthened in the 1960s and that this development has increased the political influence of those organizations, particularly the unions, which are represented in the laymen boards. (I made the same observation in my own book, *Intresseorganisationerna i dagens Sverige* [Interest organizations in Sweden today]; this is, however, disregarded by Wheeler in a rather careless polemic against me [pp. 160–62].)

The big battle between the political parties over the supplementary pensions issue at the end of the 1950s provoked a serious crisis within the TCO. According to their group interests, some unions took sides with the Social Democratic party and the LO in a demand for a compulsory pensions system enacted by law, whereas other TCO groups were in favor of the nonsocialist program for a voluntary system created through labor market settlements. The internal conflict affected all levels of the TCO, including the top leadership: the only possible solution was to abstain on the contested issue. Such a neutral position on an important matter of political contention is probably inevitable for an interest organization which has declared its political neutrality and has a membership with almost the same party composition as the whole electorate.

In this connection I think that Wheeler should have discussed the problems of political neutrality, which are highly relevant for an organization such as the TCO. These problems have become acute in the last few years, during which the TCO has taken a radical position on many issues, particularly in the fields of industrial democracy and labor law, and has formed a sort of "wage-earners' front" together with the LO. The TCO has to weigh the demands of the membership, irrespective of the standpoints of the political parties, against the risk of being identified with the Social Democratic party because of intimate cooperation with the nonneutral LO. However, Wheeler does not see this potential conflict, although it was already quite visible in the beginning of the 1970s when he was finishing his work.

The recent radicalization of the TCO can be regarded as the organization's answer to a spontaneous membership revolt against bad working conditions and imperfect industrial democracy. This is a much more fundamental fact than the relatively weak tendency toward "direct action" in Sweden: wildcat strikes, activist groups, student revolts, and so on. Unfortunately, Wheeler grossly overestimates the importance of these phenomena; he seems to believe that they still constitute a real threat to the established powers in Sweden, including the big labor market organizations. In the last chapter, "Alienation," he gives way to empty speculation about a future transformation of the solid Swedish system through youth

activism, radical school reforms, etc. He disregards the capacity of the labor market organizations to anticipate and channel popular discontent. The "wage-earners' front" is stronger than ever in Sweden today, and the organizations have the full confidence of their members.

Ethnicity and Resource Competition in Plural Societies. Edited by Leo A. Despres. The Hague: Mouton & Co., 1975. Pp. xii+221. $12.50.

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As I read this collection of papers, I could not help wondering whether the complexity of the phenomenon of ethnicity was not in part a function of the sophisticated intellects brought to bear on the problem, though to be sure other factors contribute. The ethnic relations covered in the case studies range from the most fluid relationships, with ill-defined and permeable boundaries between ethnic groups and social definitions varying with different contexts, to the most rigid, as exemplified by colonial societies in Africa. They deal with different levels of relationship, the interaction of individuals, the identities ascribed to population aggregates, and the activities of corporate or politically organized ethnic groups: they are informed by a spirit of eclecticism. This is expressed in the conclusions contributed by the editor and in his approach to the contributors, who were not asked to reflect upon a preestablished conceptual framework but invited to pursue their inquiries quite independently.

The volume is part of the World Anthropology Series designed, under the general editorship of Sol Tax, to extend anthropological perspectives by encouraging contributions from scholars representing a variety of world cultures. The papers were intended for presentation at the IXth International Congress of Anthropological and Ethnological Sciences. Contributors were invited by the editor, Leo A. Despres, to focus "comparative attention on the relationship that might exist between the genesis and persistence of ethnic boundaries, the political incorporation of ethnic populations, the organization of inter-ethnic relations, and the competition for environmental resources" (p. 3).

Four of the case studies deal with New World populations. Regina E. Holloman analyzes boundary maintenance and resource competition among the San Blas Cuna of Panama, demonstrating the utility of "a framework which focuses on interethnic boundary maintenance as related to the capacity of the ethnic system to preserve or enhance its members' competitive advantage with respect to resources" (p. 32). Norman E. Whitten analyzes the rather fluid ethnic relations of the Canelos Quechua of Ecuador among whom identifications vary with situation. He describes the manipulation of ethnic boundaries and develops the thesis that the expansion of Lowland Quechua identity must be understood "as a rational response to expanding opportunities in the money economy under the

continuance of internal colonialism in Ecuador" (p. 47). The two remaining New World case studies take up the relations between class and ethnicity. Pierre L. van den Berghe deals with this problem in Highland Peru. Drawing on his extensive comparative research into ethnic relations in Africa, the Americas, and Europe, he summarizes the "well established parameters of ethnic relations," emphasizing that the specific relationship between class and ethnicity is an empirical question to be answered in every particular case and warning against any a priori theoretical position that attributes a paramount role to either (p. 72). His study demonstrates the fluidity and indeterminacy of the Peruvian situation. Despres indicates that in Guyana, by contrast, the lines between East Indians and Africans are more sharply drawn. In this regard, his discussion of corporate ethnic organization under the spur of competition for resources is of particular interest, as are his suggested generalizations concerning the genesis and persistence of ethnic ascriptions and status identities (pp. 113–14)

Two African case studies follow Despres's selection. Onigu Otite offers three neat vignettes of ethnic relations in Nigeria in an attempt to approach ethnic differences and ethnic group conflict as a function of resource competition. The first deals with a situation of unequal access to land by ethnic groups having a symbiotic relationship between them, the second with free access to land and harmony in economic and political relationships, and the third with ethnic economic competition in an urban setting. Elliot P. Skinner presents an overview of competition within ethnic systems in Africa in the precolonial, colonial, and independent periods. There is an interesting section on strangers, written on the whole with sympathy, even though it quotes what I would regard as hostile stereotypes of non-African strangers The final case study, by Melvyn C. Goldstein, is of ethnogenesis among Tibetan refugees in South India, which he relates to the competition for resources and the role of the Tibetan "government" in exile. He accepts and demonstrates the thesis in the Tibetan case, that "the retention of traditional sociocultural patterns in new environments is a function of the advantages they yield" (p. 173).

Falling outside the framework of the case studies is a theoretical discussion by Harmannus Hoeink of the mechanisms for allocating resources, their relationship to different forms of socioracial stratification in the New World, and variables affecting the emergence and maintenance of ascriptive group boundaries. Some of the points are taken up in the editor's conclusion, but on the whole this remains a somewhat detached item, of interest in its own right.

Three main theoretical perspectives are represented in this volume. The first, with some qualification, derives from Frederick Barth, and more particularly from his introduction to the volume he edited, *Ethnic Groups and Boundaries*, in which he deals with problems central to the studies under review. His emphasis on self-identification as the critical criterion of ethnic identity and on the choices open to, and the strategies pursued by, the actors is especially relevant to situations of fluid interethnic relationships with highly permeable boundaries.

The second perspective is that of functionalism, expressed in the theory that the genesis and/or persistence of ethnic boundaries are a function of the competitive advantages they confer with respect to material resources. There are qualifications of this theory in the volume (reference to situations in which ethnicity is imposed or successful ethnic strategies become dysfunctional), but on the whole I think that there is too uncritical an acceptance of functional theories of ethnicity.

The third theory is that of plural societies defined, in a tradition deriving from J. S. Furnivall, as societies with pervasive cleavages characterized by conflict and held together by force. Skinner describes his own ethnic system model as quite different from that of the plural society and offers a point-by-point characterization of the plural society model (pp. 152–53). He happens to be totally wrong point by point. His ethnic system is in fact not to be distinguished from a plural society, and his analysis is appreciably that of traditional plural society theory.

However varied the theoretical courses pursued by the contributors, they agree in demonstrating an intimate relationship between ethnicity and competition for material resources. Curiously enough, the contributors make few references to Marxist theory.

Contradiction and Change: Emerging Patterns of Authority in a Rajasthan Village. By Anand Chakravarti. Delhi: Oxford University Press, 1975. Pp. xii+234. Rs 55.

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The "contradiction" of Dr. Chakravarti's title pertains to two sources of authority in one Rajasthan village: the tradition of command wielded by persons of Rajput descent on the one hand and, on the other, a modern ethic of equality thrust upon India by land reforms and by the institutions of local self-government. The politicians and their planners design a new form of society, enact rules to bring it about and provide sanctions and personnel, and then are helpless to do much beyond letting events take their course. Thus the "change" of the title, provoked by the "contradiction," consists of a set of events infinitely more complicated than the legislation in which it partly originates.

There are two reasons why this is so. First, one inevitable characteristic of the planning process is that planners do not know enough about the important features of the society and culture which they aim to change. This is partly in the nature of social things: they are complicated enough to make it difficult to know what really counts until after the event. Second, the fault lies in the specialized interests of planners which often conflict with local interests such as religious susceptibilities, loyalty to kinsmen, and the intrinsic value set on a style of life that is irrational from the point of view of a money-maker. Such local interests come to

be ruled out by the planners as irrelevant. Most curious of all, nothing ever seems to be evaluated until one reaches the very lowest level of implementation, to the political propensities of those who will see the plan as a resource for gaining or maintaining power.

Chakravarti, within the framework of his interest in the uses and sources of power, describes one instance in which this problem was faced. From about the mid-18th century until 1954, the community he studied was unambiguously ruled by Rajputs, a minority of warrior landholders who, in 1954, constituted 9% of a larger population of about 1,700 which included tenant farmer castes (Jat and Ahir) and a variety of specialists ranging from Brahman at the top to Untouchables at the bottom Until 1954 Rajputs owned 84% of the land: others not only worked the fields for them but also were bound to provide various kinds of service. Contact with the world of rulers and administrators and even the commercial world was the sole province of Rajputs, and what went on outside offered no threat to continued Rajput dominance. Undoubtedly, as the author convincingly illustrates through his cases, Rajput landholders not only were granted legitimacy by their subjects (in the sense that to strike back was, if not inconceivable, at least thought to be verging on the sinful), but also, on those occasions when the landholders felt obedience was not sufficiently prompt or when they simply felt like helping themselves to more of what was available, they could take the law into their own hands and have the recalcitrant subject beaten.

In 1954 there were agrarian reforms, and after these were implemented the small Rajput group owned 29% of the land—less than half their previous share but still enough to make them the major landholding group. They also lost many of the perquisites that had gone along with being a Rajput landholder. A naively materialist conclusion would be that Rajput power should therefore have become less than half of what it had been. Nothing so simple happened; there is no straight reflexive connection between economic resources and political power. The capacity to use force and fraud intervened, and the Rajputs used these devices even in the course of the land reform itself. In some cases the habit of subservience was such that subjects, despite having been made aware of their rights and despite offers of support from powerful people, nevertheless surrendered their rights as tenants and allowed the landlord to represent land which had for years been leased out to tenants as land worked by himself and his immediate family. To some extent, of course, this spinelessness was a realistic response to the realities of power: the alternative might have been a cracked skull or a house burned down, with no recourse because the landlord knew how to place bribes and how to manipulate the impersonal bureaucracy much better than did his former tenant. The book abounds in similar cases of might translating itself into right over the next decade.

But during this time the new rules which asserted the equality of citizenship against the hierarchy of caste began to take effect. They did so through the agency of individuals. One man in particular who was him-

self a Rajput became the champion of the former oppressed groups. Through his efforts less powerful people began to stand up against Rajputs, and such opposition began to be viewed as legitimate. The champion does not seem to have been any more scrupulous than his opponents in the tactics he used—he could hardly have succeeded otherwise—but, in directing people in the use of new institutions (like the local government bodies) and helping them to do so effectively, he endowed them with some measure of power.

Chakravarti tells this complicated tale through the story of two men: one is the "champion" and the other is like the hero of *The Last Hurrah*, a man of the past. By focusing upon these two men and the political resources at their disposal, the author effectively analyzes the politics of reform. There are no high flights of theory—not, indeed, to be expected when one starts at ground-level ethnography—but the author might have attempted some generalizations about tactics and strategies. Nevertheless, this book is undoubtedly a very good contribution to the literature on social change, development, and micropolitics, apart from the obvious relevance it has to regional and Third World concerns.

African Apostles: Ritual and Conversion in the Church of John Maranke. By Bennetta Jules-Rosette. Ithaca, N.Y.: Cornell University Press, 1975. Pp. 302. $9.00.

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African Apostles, by Bennetta Jules-Rosette, is a monograph in a series of interdisciplinary works on the role of symbols, myths, and rituals in social and cultural processes edited by Victor Turner. This study, by a culturally oriented sociologist, focuses on a particular Central African religious movement, the Apostolic Church of John Maranke (the Bapostolo). The Apostles' movement is one of a large number of religious movements that currently exist throughout sub-Saharan Africa. It originated more than 40 years ago in Rhodesia, has spread as far northward as Zaire, has some 150,000 members, and is notable for the "symbolic combination" of Central African and Western Christian culture in its "reasoning and ritual" (p. 190).

From 1969 to 1972 Jules-Rosette intermittently studied this movement, focusing particularly on its rituals. Her point of view was that of an "observing participant" (p. 2 and passim) who moved back and forth between various Apostolic settings in Zambia, Malawi, and Zaire (chiefly West Kasai). In 1971 she was converted to the Apostles. She claims that her membership in the movement not only provided her with a "vehicle for description" and resolved the "commitment versus objectivity . . . tension" in her research but also allowed her to "link . . . categories and action . . . symbolism and ritual" (p. 15). Her modes of inquiry included

the classical field methods of direct observation and in situ interviewing, the use of documentary data from government and Apostle archives, the taking of oral histories, and the administration of a questionnaire. In addition, she made tape recordings, transcriptions, photographs, and movies of ceremonies which she shared with the Apostles in order to elicit their explanation of ritual events and their reactions to them.

This book has certain unequivocal strengths. It is also limited in ways that are not unique to this particular study but raise larger questions about the kind of microanalytic approach that it represents.

One of the primary assets of the book is a literary one. It is written in clear, unadorned prose. And yet, although the style is lucid, by and large it is not evocative. It fails to convey anything like a sensual impression of what is either African or Christian about the movement. Rather, the author presents her materials in an excessively formalistic and intellectualized way. This is surprising in a work that professes to come from deep within the author as well as from the expressive essence of the Apostles. Only in the final chapter of the book where she describes her poignant struggle to remain an Apostle after her "silent return" to the United States and to "preach the gospel" of the movement to the people of her "tribe,"—the people of America—does emotion break through.

The best part of the book, chapter 6, entitled "The Living Ritual," contains a subtle analysis of the "delicate balance" and the "flexible," dynamic interplay between Christianity and African nativism, "the customary and the new" (p. 214), in Apostolic rules and ritual Here, Jules-Rosette rises above the all too general tendency of social scientists dealing with religious movements in developing societies to categorize them mechanically as traditional or modern, nativistic or acculturative, and to write woodenly (and often incorrectly) about syncretisms between the different cultural elements of which they are composed. The fact that she not only belonged to the Apostles but also studied them seems to have contributed to her ability to grasp the intricate and ordered, but changing way in which the "imagery of the old and the new, the concrete and the universal" (p. 187) are continuously combined and recombined in the movement.

Yet, throughout most of the book, Jules-Rosette does not demonstrate the cultural knowledgeability and sensitivity that she displays in this one chapter. She often fails to deal adequately with either the juxtaposition or shifting relationship of African and non-African elements in the movement. For example, although she writes extensively about the Apostles' weekly Sabbath "ceremony of praise," its prayers, preaching, and songs, she does no more than mention the fact that the name given to this ritual center of the movement is "kerek," the Dutch word for church. She merely mentions the possibility that the term has Afrikaaner origins without further exploration or analysis. She highlights the significant degree to which the Apostles are preoccupied with health, illness, and healing. (She herself was confirmed as an Apostolic healer.) But she does not discuss the significant degree to which this set of concerns is

characteristic of most Central African religious movements, and beyond that, one of the fundamental magico-religious foci of the entire culture. Nor does she suggest how the Apostles differ from other African movements in this respect. Their proscriptions against modern Western medical procedures and medicinal drugs, for example, distinguish them from virtually all other religious movements in Zaire, as does the exceptional degree to which Apostle symbols and rituals are infused with Christian imagery and thought forms. Many other examples could be given of Jules-Rosette's descriptive, analytic, and interpretative omissions: in her treatment of the symbolic meaning of various colors used by the movement (red and white, for instance); in her account of "keti" (prophetic examination) and confession (pp. 172–83), and in her presentation of the role of the palaver (pp. 156–66). Although they are elaborated by the Apostles in particular ways, each of these symbols and rituals is a core component of many other Central African movements and of the common cultural tradition from which they have emerged.

The paradox of Bennetta Jules-Rosette's microdynamic approach is that it is less refined and reflexive than it could have been had the perspective been macroscopically comparative. Her book leaves one with a fragmented understanding of the movement. It is a kaleidoscope of proximate detail that may have an everyday reality about it but in the end does not give an integrated and informative ethnographic account of the movement.

Implicit Meanings: Essays in Anthropology. By Mary Douglas. Boston: Routledge & Kegan Paul, 1975. Pp. xxi+325. $23.50.

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Ever humorous, in her collected essays Mary Douglas describes herself at work as though she were tidying up around the house: here she lifts a veil; there she draws a curtain; elsewhere she finds a bit of dirt. What meaning lies hidden in these silent actions? Is she spring cleaning in the parlors of magic and religion?

Briefly, Douglas is bent on discovering certain regularities, as fundamental as they are broad and simple, which should provide the foundations for her larger project of developing a comparative sociology of knowledge. A number of these 17 essays (about jokes, meals, body language, animals sacred and défiled, distinctions between humans and beasts, environmental pollution and the ecology movement) exemplify her sense that the strategic task at present is to analyze delimited domains and to show their internal coherence as well as the operation of their social determinants. Her goal in these explorations is to demystify the human condition by reaching beneath the surface and getting to the bottom of things and by looking behind the screen of appearances at the self-regard-

ing passions and the interactions which generate social and symbolic forms. In this "unmasking" enterprise Douglas locates herself historically as an inveterate follower of Durkheim, whose central message about the social determination of knowledge had an import, if not an impact, as significant as that of either Freud or Marx.

Once at work in an analytical domain, Douglas moves (as if through the rooms of a house) from context to context until she notices certain formal patterns which recur here, there, and everywhere. Her sense is that such formal patterns emerge into plain view only by juxtaposing diverse varieties of evidence which, by social convention, are normally segregated and viewed as unrelated. In short, the myriad isomorphisms of social and symbolic forms, pieced together as they have been, neither explicitly inform people's ordinary language nor guide their intentional conduct in the everyday world; just as the title says, they are "Implicit Meanings."

The first seven essays on "The Implicit" explore the interface between explicit and implicit knowledge; the margins between the conscious and the suppressed; the boundaries where a society's "foregrounded" conventional wisdom intersects with its "backgrounded" information (which happens not to fit with received ideas). A prohibited animal, for instance, was the star of Douglas's seminal work, *Purity and Danger* (1966). This anomalous creature, the pangolin (pictured on p. 34), is regarded by the Lele of Africa "as a scaly fish-like monster that ought to abide in the waters, but creeps on the land"; moreover, in their view, it combines a human characteristic with those of a fish and a lizard in that "like humans, it gives birth to one child at a time" (p. 33). For the Lele, pangolins are clearly weird. Thus prohibited animals, taboos in general, jokes, rites of passage, all are similar in that they focus on margins and they characteristically proceed by way of puzzling riddles, baffling paradoxes, and anomalous categories.

It is in her middle section of six "Critical Essays" that Douglas locates her enterprise within a reference group of contemporary significant others. Her review essays discuss a number of works by the French anthropologists, Claude Lévi-Strauss, Louis Dumont, and Missions Griaule, plus Victor Turner, Basil Bernstein, and Carlos Castaneda. Her comparative sociology of knowledge, it becomes clear, represents an effort to achieve a synthesis of French structuralism with its virtuoso analyses of "the marvellous world of forms" and British empiricism with its more mundane flair for uncovering "the effect of men's ambitions and remorse on society and the cosmos" (p. 140).

What Douglas is grappling with here is the antithesis between the "front-stage" bias—that society is designed so as to produce its visible social and symbolic forms—and the "back-stage" bias—that transactions and exchanges alone are real in contrast with the illusory epiphenomena of social and symbolic forms. As instances of the former she lumps David Schneider and ethnoscience together (curious bedfellows, to be sure), whereas she exemplifies the latter by "Sidney and Beatrice Webb [who] are reported to have enjoyed an evening at Covent Garden calculating

from the average box-office takings what the dancers earned" (p. 120). Though in theory she wants it both ways, at once front-stage and back-stage, in practice she never tacks back and forth reflexively between thought and action as she analyzes a specific domain. Instead, Douglas presents each analysis as if she had done her work in two distinct stages: first, in a flurry of front-stage activity she follows the French in mapping out the duplicated and reduplicated configuration of forms which pervade the social and symbolic realms; second, in her back-stage operation she returns to her British commonsense perceptions and waffles between (*a*) reducing symbolic forms to social forms and (*b*) pulling back the veil to reveal that all along it was the burning of the self-regarding passions—competition, rivalry, envy, hate, love, ambition—which (and here she stands Plato on his head) cast the shadowy formal patterns on the wall.

In her initial front-stage task, then, she develops the structural analysis of a system of thought. The goal of these structural analyses is not to reduce the various instances of a particular form to a privileged exemplar but rather to grant them all equal status as manifestations of the same pattern. Douglas, for example, makes this point as follows in concluding her essay, "Jokes": "The exercise of tracing the analogies drawn in joke rites gives additional meaning to Kandinsky's famous saying that the impact of an acute triangle on a circle produces an effect no less powerful than the finger of God touching the finger of Adam in Michelangelo's famous fresco" (p. 112). The underlying basis for the deep resemblance between formalism in art and the structuralist "exercise in tracing the analogies drawn in joke rites" is that her sociology of knowledge aims less at the textured characterization of particulars (as I would prefer) than at the formal shape of things known. Indeed, as they appear in her essays, these shapes are usually geometric, and they consistently lend themselves to visual representation, either as circles or as a rank-ordered series of parallel lines. Trendy in her invocation of linguistics and communication theory, Douglas tells us that the reason such formal homologies reproduce themselves so widely and so often is that human societies, as if propelled by their own nature, attempt to achieve communicative consistency by broadcasting the same message on all channels.

Her second chore back-stage is highlighted by the trick of demystification in which Douglas reduces the front-stage world of forms to their so-called generative matrices either (*a*) in social structural forms or (*b*) in the passionate dynamics of human interactions. Let me consider these two reductionistic strategies by making brief critical remarks on her four final essays ("The *a priori* in Nature") in which she discusses cultural truths that are so axiomatic for a people's world view that they appear "self-evident" and therefore remain implicit and beyond scrutiny.

1. In "Environments at Risk" she develops an argument initiated in the first section (in "Couvade and Menstruation" and "Heathen Darkness" which belong in the final section) and says that if ideas of "pollution" and "purity" and "danger" in other societies are socially determined, then the same thing must be true in our own society as well. Much

as we might wish to think that the present-day ecology movement rests on the bedrock of scientific truth, she says, it is shaped rather more by such "verbal weapons of control" as arguments which appeal either to the scarcity of time or money or to the calamities which befall those who risk the polluting consequences of daring to violate the order of God or nature. Fine as they were, she then jettisons her insights about the rhetorical shape of "verbal weapons" when she says with good cheer, "I have no doubt that our earliest cave ancestress heard the same, when she wanted a new skirt or breakfast in bed" (p. 236). So, all people in all times and in all places use rhetoric to persuade others into taking their side. Why not? Yet, are we now to believe that we have reached rock bottom, the place where, in its last analysis, Douglas's enterprise rests at the source of social and symbolic forms?

2. In "Deciphering a Meal" concentric circles represent degrees of elaboration in meals (symbolic form) which in turn correspond with degrees of social distance (social form). Here, she uses a "type A" reductionist strategy where symbolic forms are projections of social forms. Not only does her analysis ignore the taste of food (surely a critical ingredient of any study of meals), but she never explores "social distance," the actual subject of her essay, into such other Goffmanesque realms as greetings, deference and demeanor, the presentation of self. In the latter case, then, her interpretive movement should be reflexive: from symbolic form (meals) through social form (distance) and back to other symbolic forms which exemplify social distance.

3. In "Self-Evidence" the areas of overlap among circles (pictured on pp. 292, 294, 299, 300) represent the classificatory boundaries between humans and animals (symbolic form) which in turn correspond with the boundaries between "in-group" and "out-group" (social form). She is at her best in her four-way comparison of Africa, New Guinea, Thailand, and the Old Testament. Here, however, she uses a "type B" reductionist strategy and finds that marriage rules underlie social and symbolic forms. The sense in which marriage rules comprise a level of analysis different from that of social forms is unclear. It is also perplexing to discover that marriage rules, for Lévi-Strauss the point of departure par excellence, have become, for Douglas, the final end point of analysis Again, has she reached rock bottom in finishing where her predecessors started?

4. In "In the Nature of Things" graphs (pictured on pp. 218, 220, 221, 224) represent the hydraulic relations between the ratio of public versus private classification (symbolic form) and the ratio of control by others versus control over others (human interaction beneath social form). Useful enough as a heuristic, this graphic idea is misleading only if it suggests the possibility (as it does for Douglas) of a universal taxonomy of classification systems or a worldwide scale of degrees of social control. The comparative sociology of knowledge fares better in the simultaneous analysis of four rather than 400 societies.

Finally, she claims to straddle, not only front-stage and back-stage, but also the related, yet distinct, polar stances of symbolic anthropology

which divide (*a*) those who think that symbols express something else which generates them and to which, in the last analysis, they refer from (*b*) those who consider that symbols are so deeply constitutive of social reality that it is impossible to separate their expression from what is expressed. One might, for instance, follow Freud and Victor Turner in defining a symbol as something which stands for something else; one might also prefer to follow Weber and Clifford Geertz in defining a symbol as something which in and of itself embodies a particular conception of reality. In "Do Dogs Laugh" Douglas attempts to fuse these two opposed views simply by saying that "since the body is mediating the relevant social structure, it does the work of communicating by becoming an [*a*] image of the total social situation as perceived, and [*b*] the acceptable tender in the exchanges which constitute it" (p. 87). In principle she wants to think that symbols are simultaneously expressive and constitutive; in fact, this issue, significant as it is, remains confused and unresolved throughout her actual analyses.

While it ignores (to its detriment) intellectual strands from the United States (Geertz) and Germany (Karl Mannheim), this collection deals with genuinely vexing problems of comparison in ways, however confused, that are often insightful and always stimulating. Though the volume's price is lamentably high, Douglas should clearly be read by all with a wide-ranging curiosity and especially by those concerned with symbolic anthropology and the sociology of knowledge.

The Character of Kinship. Edited by Jack Goody. New York and London: Cambridge University Press, 1974. Pp. xii+251. $10.95.

Nelson H. H. Graburn

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This, the latest collection of essays under the editorship of Jack Goody, is a festschrift for Meyer Fortes on the occasion of his retirement. The authors were selected from Fortes's students and colleagues, and the theme of the book is the reconsideration of some general problems in the anthropology of kinship.

The first section, "Kinship and Descent," is opened by Frederick Barth who refines his ideas on endogamy as a feature of Middle Eastern kinship systems. He points out that the actors carefully maintain the distinction between affinals and consanguineals, as well as between agnatic and matrilateral relatives. He suggests that Fortes and his ilk are wrong in asserting that "segmentary structure is a logical entailment of unilineal descent" and once more asserts that the kinship systems of this area are generated by the politicoeconomic system and not vice versa.

The next two papers on kinship in New Guinea differ almost diametrically. Andrew Strathern suggests that "descent" may as well be a creation of neighborhood proximity as of biological reckoning. He shows how

coresidents *become* kinsmen after three or four generations and suggests that this is an adaptation to the peculiarities of population distribution in the area. He goes on to show that in "his" area, kinship (descent) and land (coresidence) are intertranslated through the medium of food; thus the consubstantiality (see later) of having eaten from the same land is symbolically translated into the higher order of consanguinity itself. Jean La Fontaine, on the other hand, defends the original, Africa-generated Fortesian models which have been attacked as inapplicable to New Guinea. She shows quite convincingly that the apparent differences arise from post–World War II ethnographers' interests in process and variation and asserts that the pure corporate model was an idea rather than a statistical fact in Africa as elsewhere. "What is involved here is a difference in emphasis which can be explained in terms of the history of anthropological theory rather than the geography of anthropological fieldwork" (p. 41). This section concludes with a minor article by Edmund Leach in which he shows that the English bourgeoisie from which he himself descended used matrifiliation to link with patrilineal ascendants when it suited them, thus adapting a basically bilateral system to one with a patrilineal idea.

The section entitled "Nature of Kinship" is the most interesting to me. It starts with a paper by John Barnes on the etic and emic problems of the anthropologists' view of parenthood. Barnes suggests that motherhood is less culturally variable because it is biologically anchored, whereas fatherhood is more open to differential symbolic loading. Most of Barnes's paper takes off from Schneider's attempts to show the relations between scientific and fold models of kinship in American society, but I feel that he falls into the very trap that he feels ensnared Schneider, that is, lack of cultural relativism. Maurice Bloch uses his Merina (Madagascar) and other ethnographic materials to illustrate the longer- and shorter-term moral commitments of kinship and pseudo kinship, refining Fortes's position that *amity*, prescriptive altruism, is the basis of kinship. Following Sahlins, Bloch demonstrates the reciprocal nature of day-to-day tasks and the longer-term uncalculated transactions between close kinsmen, concluding that kinship is therefore a flexible basis for many adaptive relationships, whereas more instrumental ties are brittle and short term.

Julian Pitt-Rivers's paper, "The Kith and the Kin," again starts with the basically moral nature of kinship and the notion of amity and parallels the latter with Sahlins's notion of generalized reciprocity and Schneider's idea of diffuse enduring solidarity. But he identifies the symbolic fundament of kinship relationships as that of *consubstantiality*. He then examines and classifies various types of institutionalized relationships, such as adoptive and ritual kinship, friendship, and marriage, in what is perhaps the most stimulating paper in the book. Although Derek Freeman's "Kinship, Attachment Behavior and the Primary Bond" begins the section "Nature of the Family," it belongs logically with the previous contributions, as it also is concerned with the basis of kinship. Freeman draws our attention to recent studies of child development and instinctive behavior

which show that during the period from the third to the fourteenth month of life monotropic attachments (usually to the mother) take place, creating overwhelming but ambivalent relationships which are perhaps the basis of amity and of the content of all other subsequent relationships.

Raymond Smith's reexamination of "The Matrifocal Family" reminds us that when he first used the term (1956), following Fortes's work on the Ashanti domestic group cycle (1949), he "intended to convey that it is women *in their roles as mothers* who come to be the *focus* of relationships, rather than head of the household as such" (p. 125), and he berates others who have consistently misused the term ever since. In his own Caribbean data he demonstrates a continuing bilateral emphasis, and he diminishes the distinction made by others between legal and extralegal marriages for lower-class partners. Grace Harris's short paper examines Aeschylus's *Oresteia* to show that complementary filiation is the line of inheritance of mystical attributes and points out that the corresponding forms in patrilineal and matrilineal societies are not symmetrical, because in both cases men hold the jural-political power.

Roger Abrahams opens the section "Marriage and Affinal Roles" with a provocative paper on the levirate, contrasting it with widow-inheritance, and illuminating the structural relevance of such concepts as the continuity of office, the equivalence of siblings, and the need to maintain individual identity. This is followed by Jack Goody's "Polygyny, Economy and the Role of Women." He debunks various beliefs such as polygyny's being strongly correlated with women's horticulture and being negatively correlated with urbanism. He reasserts the lay belief that "sex alone must play a powerful role in polygyny," and concludes that it is not polygyny that needs explaining "but its absence, i.e., monogamy" and that it is the latter, the rarer phenomenon, that is caused by economic factors.

The reader will be delighted to find that the brightest jewel in the book is left until last: S. J. Tambiah's "From Varna to Caste through Mixed Unions." This monograph-length contribution—which should really form the foundation of a separate book—attempts to explain the categorization and hierarchy of castes as deriving from the rules of *anuloma* hypergamous permitted marriages and the disapproved *pratiloma* hypergamous unions. The theory does not pretend to represent history but does account for the structural scheme in a way consonant with the historical laws of caste. From this he goes on to examine the different kinds and notions of pollution and concludes with "the marriage of purity and power."

This important volume demonstrates the continuing richness of the "descent theory" tradition and it is surprisingly fascinating for a book which mentions Lévi-Strauss in only two passages! We are reminded of Fortes's particular contributions to our understanding of kinship and descent: his clarification of corporate descent groups, in their familial and jural-political aspects; the useful notion of complementary filiation; the dynamic aspects of kinship in the developmental cycle of domestic groups; and the irreducible nature of kinship relations. In addition to motivating

us to reread Fortes's *Kinship and the Social Order* (1969), the papers make us aware of the importance of the works of Edmund Leach and David Schneider in questioning, modifying, and making even more useful these central notions about kinship and descent.

Managers and Workers: Origins of the New Factory System in the United States, 1880–1920. By Daniel Nelson. Madison: University of Wisconsin Press, 1975. Pp. x+234.

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While historian Daniel Nelson must be commended for his meticulous search through company archives, trade journals, and industry histories to trace the evolution of the American factory system in the crucial years between 1880 and 1920, the entire enterprise seems somewhat detached from reality. For Nelson made a conscious decision to ignore any treatment of trade unions, wages, hours, working conditions (aside from certain safety issues), and unemployment. "This selectivity," Nelson argues in the preface, "has made it possible to focus on the heart of the subject, the changing relationship between the factory manager and the factory employees between 1880 and 1920" (p. x). But one may just as easily argue that in so doing Nelson has in fact *ignored* the heart of the subject and concentrated instead on its periphery.

After all, the years 1880–1920 were not only vital for the evolution of the American factory system but also spanned the greatest period of labor unrest in American history. A casual listing of the highlights recalls the class violence of the period: following the strikes in the Pennsylvania coal fields in the 1870s associated with the Molly Maguires and the great railroad strikes of 1877 came the railroad strikes of the 1880s led by the Knights of Labor, the Haymarket Square Riot of 1886, the battle of Homstead, the Pullman strike, the great coal strike of 1902, the Ludlow massacre, the struggles of the IWW in the west and the triumphant Wobbly textile strike in Lawrence, Massachusetts, and on and on.

This was the age of Pinkerton armies, of state militia and federal troops used as strikebreakers, of employer blacklists and yellow-dog contracts, of injunctions and conspiracy laws used against trade unions. In the mid-1890s the average wage in manufacturing was $400 a year, the work week generally ranged from 54 to 63 hours—and longer in steel, textiles, and tenement-house sweatshops—and in the depression year of 1893 3 million workers were unemployed. Nelson discusses none of these things systematically. The term "unemployment," for example, does not even appear in the index of Nelson's book. Undoubtedly because his major sources were company archives and histories, he slowly and perhaps unconsciously sank into a managerial *Weltanschauung*. Thus, while the author neglects

the serious problems of unemployment during this period, he repeatedly turns to company problems of labor shortages.

In any case, it is this central reality—of labor struggle, of low wages and long hours, of employment insecurity, and dismal sweatshop conditions—that Nelson largely overlooks. It is not that he has completely ignored the larger social and political environment of the factory. He has written an extremely competent chapter on the impact of Progressive era factory legislation on the industrial environment. He examines and rejects, correctly, I believe, the thesis of James Weinstein and other radical historians that factory regulation beginning in the Progressive era served mainly business interests and was enacted essentially for that reason.

Nelson also provides an intelligent assessment of the federal government's role in altering traditional labor-management relations during World War I. And one of his best essays treats the recruitment of the factory work force in this early period—the ways in which European workers were drawn to this country and the essentially *Gemeinschaft* recruitment techniques utilized by foremen, relatives, friends, and countrymen. He documents the enormous proportion of foreign-born workers in the American working class at this time, describes the massive labor turnover in these early factories, and illustrates the pattern of the invasion and succession of diverse immigrant groups in a wide variety of industries.

Nevertheless, in this turbulent period of American labor history Nelson often dwells on esoteric asides. While ignoring wages, hours, conditions, and trade unions, he devotes an eighth of the book to a description of so-called welfare work, that meager assortment of ameliorative measures which paternalistic management provided for their workers—libraries, social clubs, adult education classes, athletic programs, factory "rest rooms," informal pension and insurance plans—almost all firmly under management control, often introduced as an economic investment and withdrawn if it didn't pay off or if it was rejected by an "unappreciative" work force.

The author's treatment of changing factory architecture, technological changes, the decline of the foreman's power, the role of work councils or employee representation plans, those pseudo-participative devices introduced by management after 1900, are all carefully researched but somehow abstracted from the daily reality of factory life.

Nelson's discussion of the rise of scientific management is likewise carefully researched but unsatisfying. If one knew nothing about Taylorism but what is contained in Taylor's account, one would never know what the fuss is all about. Strangely, in a book on the relationship between management and workers, Nelson stresses Taylor's technical innovations in the factory, and we get only a fuzzy description of Taylor's approach to labor motivation and labor control. One gets a clearer overview of the ideology, theory, functions, and implications of Taylorism from almost any industrial sociology text, not to mention Harry Braverman's brilliant analysis in *Labor and Monopoly Capital* (New York: Monthly Review Press, 1974). While Nelson concludes that scientific management had

minimal effect on the factory work force, Braverman argues more persuasively that Taylor and his ideological descendants successfully laid the groundwork in the effort to extend managerial control, not only over the worker's presence in the factory, the hours he remained there, or his deportment while on the factory floor, but also over the exact manner in which his work was organized and performed.

Just when one tires of the sterile and historically empty generalizations sociologists often bandy about one encounters a book such as this, deadpan historical empiricism which takes rich data and merely lets them lie, without any attempt to link them to a body of theory or even give them some broader interpretation. At such times one almost craves an obvious, even a facile, generalization. That these data are a theoretical goldmine is amply illustrated in the work of Katherine Stone ("The Origins of Job Structures in the Steel Industry," *Review of Radical Political Economics*, vol. 6 [Summer 1974]) who, concentrating on the American steel industry, covers the same time period and material as Nelson covers but makes sense of this material by integrating it into a theory of the consolidation of capitalist power. During the late 19th century and in some cases until World War I, in steel and many machinery factories of the northeast, production took place according to a system of inside contracting in which groups of skilled workers and their unskilled assistants contracted with management to produce a part or component of the finished product for the company, using the company's plant, tools, equipment, and materials, but retaining full control over the organization of their work. This was an early form of workers' control in the factory that has reappeared in the self-managed factories of Yugoslavia and the workplace experiments in Scandinavia. Stone perceptively shows how the steel industry, in its drive to extend and finally achieve its monopoly of power in the mills, abolished inside contracting after crushing the Homstead strike, consciously relegated decision making to higher managerial levels thus creating a strict dichotomy between mental and manual work, mechanized operation on the industry's own terms which destroyed the craft of highly skilled steel workers, introduced incentive wages and artificial job hierarchies to divide the work force against itself, and offered welfare policies to co-opt the workers and reduce labor turnover. Nelson's book contains the raw material for an examination of this thesis, but he never marshals his data in a theoretical direction.

The book is markedly nonideological, which sounds like praise but in this case really isn't. Ideology at least suggests theory and some passion for one's subject, but this book lacks both. Nelson describes these early factories, scenes of such blood, sweat, and tears, with measured phrases, careful footnoting, and antiseptic detachment. Such tidy scholarship, which eschews every bit of passion or compassion for the subject, ultimately obscures the factory reality rather than reveals it.

The Emergence of a UAW Local, 1936–1939: A Study in Class and Culture. By Peter Friedlander. Pittsburgh: University of Pittsburgh Press, 1975. Pp. xxxiii+155. $8.95.

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This detailed reconstruction of the four initial years in the organization of a local union during the massive upsurge of industrial unionism in the late 1930s is a splendid social history. It is also an astute, perceptive contribution to theoretical controversy about working-class consciousness, bureaucracy, ethnicity and class, rank and file labor radicalism and leadership, and social change.

The memories of Edmund Kord, one of the earliest union advocates in a small-parts plant in the Polish enclave of Hamtramck near Detroit, form the basis of Peter Friedlander's study. Friedlander has gone beyond recording another good oral history. He discussed his interpretations with Kord as the work progressed. The result is a collaboration, strengthened by Kord's keen eye and recall and by Friedlander's continuing dialogue.

In 1936, when he began talking about unionizing to fellow workers, Kord was a college student supporting himself by working at the parts plant, which employed mainly first- and second-generation eastern Europeans. The immediate stimulus to organization of a union was less hard times than new hopes. The New Deal, the election of a pro-labor mayor in Hamtramck, and the wave of successful union drives and sit-down strikes led many workers to overcome their fears and isolation. Friedlander gives a meticulous, department-by-department, group-by-group account of how people joined the union. Second-generation Poles had lost the deferential habits of the older immigrants still in the grip of peasant culture and were among the first to join. Very young Polish workers, acting through neighborhood gang ties in joining virtually as a group, supported the union but were enthusiastic wildcat strikers later. Appalachians were slow to join and then did so as individuals.

Friedlander presents history as a process of self-formation by individuals, groups, and classes. Moments in the process appear as structures, which in turn influence future actions. Different subgroups among workers, defined by Friedlander in terms of a common "praxis," entered into the process in distinct ways. As the local union grew, shifts in factory and union social structure were related to changing styles of activity. A dialectical relationship is revealed, in which both causality and meaningful action have their place. Resistance by one key worker to overt threats from the boss about joining the union crystallized a period of initial low-key organizing. Suddenly there was a decline in fear, a shift in mood, and the opening up of new possibilities for action and a new way of viewing the world. The balance of power shifted, although to a lesser degree than everyone except a few union officials believed, and union membership grew.

In making the union, workers were also remaking themselves. To do that they needed the power of organization. The process of achieving that, in the form of the union, also shaped the leadership and later structure of the union. Friedlander insists bureaucracy in the union grew out of the workers' situation and was not imposed by willful would-be bureaucrats. A leadership which accepted distinctions between legitimate and illegitimate issues consolidated its power, Friedlander argues, because it was more "historically articulate" than other groups, such as the rough and ready wildcat strikers for whom the only question was the possibility of victory.

Power of the workers required, in the combative situation, power over the workers, corraling the reluctant and disciplining those who sought to undermine the union. The union also gained what Friedlander describes as "almost undisputed power in the shop and to a remarkable degree usurped the authority of the management to control the process of production" (p. 80). Seniority, pay increases, and union recognition were among the first contractual victories, but the motivating ideals were as much democracy and justice as narrow utilitarianism. Friedlander does not give as much weight to the particular institutional forms, such as a binding contract and a grievance procedure, as do other observers in explaining the later decline in democracy and radicalism in the union.

Friedlander's history is best when it reveals how the divergent groups of workers interacted in creating a new union which embraced them all and was responsible for new conditions of work, a new sense of themselves, and a new outlook on their world. It is weakest when it describes the particular historical limits and possibilities which shaped the workers' own creative action. This is largely because he sees everything, including the foundation of a much more bureaucratic and conservative postwar union, springing from the activities of workers on the shop floor. However other influences, national and international politics as well as the particular course of American capitalism, affected the union. The action of other groups in society, capitalist politicians, managers, and others, along with the action of rank and file workers at the level of the local factory, molded the union and the class. Workers were not simply determined by preexisting external forces. As Friedlander shows so dramatically, they were active creators of their lives, work, and consciousness. However, they were not alone in forming themselves, their organization, or their future.

The Job Corps: A Social Experiment That Works. By Sar Levitan and Benjamin H. Johnson. Baltimore: Johns Hopkins Press, 1976. Pp. viii+118. $8.00 (cloth); $2.95 (paper).

Miriam Johnson
Olympus Research Corporation

It doesn't live up to its promise, it doesn't solve the problem, it's costly, but it works for some people. This is, perhaps, the bottom-line assessment

of nearly every manpower program designed to rearrange the position of individuals in the employment queue.

Like an emaciated ghost, the Job Corps has, almost alone, survived the dismantling and decentralizing of the national manpower programs associated with the war on poverty. The 10-year history of this unique youth residency program and its present status is traced in this workmanlike and concise overview. For administrators, the book provides the necessary history, statistics, and recommendations that characterize most evaluations. However, a reader seeking a glimpse of a Job Corps center from the inside would look in vain. No hint of texture or flavor, no first-hand impressions creep into its pages. It doesn't come alive as a human document, but it does capture the program's dramatic history.

That the Job Corps has survived at all and, in the authors' words, "become an apparently permanent and surprisingly effective program" (p. 103) is in itself startling. Highly visible and controversial, it has been subjected to emotional public and press attack, its demolition was an avowed promise made by Richard Nixon in the 1968 presidential campaign, and at least one evaluation estimated the benefit-cost ratio at a dismal 0.2/1.

As early as 1958 Hubert Humphrey advanced the notion of a youth conservation program which was defeated by Congress. The severe increase in youth unemployment in the sixties, coupled with the startling announcement by the armed forces that they were rejecting one out of every three potential draftees because of physical and mental deficiencies, prompted acceptance of the Job Corps as part of the Johnson antipoverty legislation in 1964.

In the 10-year period under scrutiny, the program has undergone immense changes in philosophy, administration, scope, and design. It has, most important, sustained severe budget cuts. At its peak, in 1967, 42,000 disadvantaged youths between the ages of roughly 16 and 21 were disbursed among 123 centers. By mid 1974 the enrollment was down 50% to 21,000 youths, and there were 60 facilities available with a capacity for 22,000 enrollees. A total of a half-million youths have passed through the program. Although traditionally underenrolled, the tightened armed forces acceptance standards coupled with high unemployment rates have resulted in a Job Corps waiting list for the first time since its inception.

Installations vary in clientele, in location, and in types of training and residential programs. However, all the centers provide supportive services, basic classroom education, vocational training, individual and group counseling, and planned recreational programs. Enrollees live in dormitories and participate in the self-governing processes as well as in the housekeeping tasks. Corpsmen and corpswomen are paid a modest stipend.

The litany of program pitfalls and weaknesses delineated by the authors in the 10-year Job Corps history echo and epitomize the stumbling blocks that have strewn the paths of almost all manpower programs directed toward "changing the man." The Job Corps overpromised and oversimplified. Its designers believed that the removal of youths from their

poverty-stricken ghetto environments would, in short order. produce well-adjusted, highly motivated individuals, given the right mix of concentrated schooling, vocational training, and fresh air. Instead, only 26% were completers. As with so many other manpower programs, the Job Corps was confronted with high dropout rates. In this regard, Levitan and Johnson explore the questions of goals, target population, the recruiting and selecting process and the "creaming" bugaboo. In addition to the high dropout rates, the public employment service was repeatedly unable to carry out an adequate recruitment, counseling, and placement program for this segment of the population. (The placement rates of 86% are startlingly high, but the authors suggest that the statistics may, for various reasons, be inflated.) Nevertheless, considerable benefits are noted, particularly for those who completed the program.

The successful wedding between unions and the Job Corps is unique in manpower annals. Unions operate a preapprenticeship program in every Job Corps center with phenomenal success and are actively involved in providing training personnel, recruitment, and placement of enrollees on high-wage jobs. The Job Corps has also made a major contribution to the development and dissemination of new techniques for educating disadvantaged youths which have been tested by the army and public school systems with remarkable results

The authors conclude that despite the evidence of waning government interest in the program it appears to be well entrenched and is not likely to be totally eliminated. It is the only program specifically directed at the most work-alienated segment of society with the highest unemployment rates.

The Descriptive Analysis of Power. By Jack H. Nagel. New Haven, Conn.: Yale University Press, 1976. Pp. xi+200. $11.50.

Power: A Radical View. By Steven Lukes. London: Macmillan Co., 1975. Pp. 64. £.75.

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There is agreement, by and large, that the ingredients of power (and, by extension, each of its correlates) are three: (*a*) acts of participants in the power relationship; (*b*) "structural variables," that is, the institutions and procedures within which the acts take place; and (*c*) outcomes, that is, events or nonevents that are the product of power exercise. Among theorists of power these ingredients are treated somewhat differently. "Behaviorists," for instance, focus mainly upon the acts of the participants: who did what to whom with what results? "Structuralists," on the other hand, focus mainly upon the environmental conditions that circumscribe the actors and thus constrain the outcomes of their interaction.

Unlike most others, Jack Nagel is primarily concerned with outcomes and their causes, arguing that "power [is] the causation of outcomes by preferences," the latter defined as "a *disposition* to make certain choices, not the act of choice itself" (p. 24; emphasis in original). The causal variable, he insists, is the preference, not the actor: B's response is an outcome which, if preferred by A, is indirectly caused by A. From this point of departure Nagel moves to his main concern, the measurement of power. In his own words, ". . . If power relations are a subset of causal relations, then any measure of causation can measure power. Path analysis [loosely, mathematical or diagrammatic tracing of relationships among causal variables] provides measures of causal effect. Therefore, coefficients from path analysis can measure power. Specifically, an actor's power over an outcome is indicated by appropriate path measures of the causal effect of his preferences on the outcome" (pp. 83–84). As Nagel points out, path analysis has proved useful to the study of certain quantitative problems in genetics and econometrics, among other disciplines. Its applicability to the study of power is more problematical. For while path analysis can be a handy device for identifying various possible chains of causation, for full effectiveness it requires kinds and amounts of quantitative data that are, and will probably continue to be, difficult, perhaps impossible, to generate. Nagel concedes as much, even as he argues for the technique's employment:

> This is not to say such research will be easy. Estimates of coefficients . would prove nothing unless the causal ordering can be substantiated. Establishing precedents among preference variables is a grave difficulty, especially when influence over preferences is accomplished quickly and continuously. [P. 105] Path coefficient estimates are entirely relative to the causal model employed in analyzing the data; and more than one model can fit the same data, by satisfying test equations. Therefore, power measures obtained by estimating a single model can be practically worthless. [P. 115] Measurement and observation of preferences will be a fundamental difficulty in the study of power, severely restricting outcomes over which power can be measured. [P. 122] This section illustrates the use of empirical data pertaining to constituency influence on members of Congress in a path analysis . . . the results demonstrate the potential inconclusiveness of path analytic causal inference, as two models are compatible with the data. [P. 125]

I concede cheerfully that Nagel's conception of power is superior in most respects to that of the behaviorists. For one thing, his formulation captures, as theirs does not, phenomena such as production of *un*intended effects, anticipated reactions, and non–decision making. Moreover, because Nagel focuses solely on whether B responds in accordance with A's preferences, regardless of *why* B does so, his conception obviates the need to distinguish as behaviorists do between power and each of its correlates, for example, authority, influence. Although this feature of Nagel's conception simplifies empirical investigation and facilitates measurement, it often will be accomplished at the expense of analytical precision.

Nagel's departure from the behaviorist point of view is, in any case, less extensive than it first seems. As behaviorists do, he adopts an "instrumentalist" stance, assuming that all outcomes are produced by the interplay of A and B (and possibly others who intervene between the two). Nagel thus ignores, as the behaviorists also ignore, the reality that at least some outcomes are the product of structural imperatives; that is, they occur in spite of rather than because of the actors. To the degree this is true, of course, Nagel's conception of power is seriously defective.

A decade hence scholars of power may celebrate Nagel's attempt to quantify power as truly path breaking. They may, on the other hand, consign it to a footnote or a lengthy bibliography of failed attempts to measure what proves to be largely immeasurable. Whatever the case, the reviewer of today must give Nagel his due—and that is considerable. With laudable rigor and with a sure grasp of the work of those who have preceded him, he has truly spoken to the condition of those of us who are, as he puts it, "persuaded that we [power analysts] now need less conceptualizing and more true theorizing and theory testing" (p. 179).

Whereas Nagel's primary concern is the measurement or "descriptive analysis" of power, Steven Lukes's focus is theoretical Summarized in his own words, his argument is that "the pluralists' view was indeed inadequate for the reasons Bachrach and Baratz advance, and that their view gets further, but that it in turn does not get far enough and is in need of radical toughening. My strategy will be to sketch three conceptual maps, which will . . . reveal the distinguishing features of these three views of power: that is, the view of the pluralists (. . . the one-dimensional view); the view of their critics (. . . the two-dimensional view); and a third view of power (. . . the three-dimensional view)" (p. 10).

Exemplified by the writings of Robert A. Dahl and his students, the one-dimensional view "involves a focus on *behavior* in the making of *decisions* on *issues* over which there is an observable *conflict* of (subjective) *interests,* seen as express policy preferences, revealed by political participation" (p. 15; emphasis in original). The two-dimensional view, embodied in the work of Bachrach and Baratz (among others), "involves a *qualified critique* of the *behavioral focus* of the first view (I say qualified because it is still assumed that nondecision-making is a form of decision-making) and it allows for consideration of the ways in which *decisions* are prevented from being taken on *potential issues* over which there is an observable *conflict* of (subjective) *interests*, seen as embodied in express policy preferences and sub-political grievances" (p. 20; emphasis in original).

The essence of the three-dimensional view is revealed through exposure of certain "inadequacies" in Bachrach and Baratz's analysis. First, says Lukes, "In trying to assimilate all cases of exclusion of potential issues from the political agenda to the paradigm of a decision, it gives a misleading picture of the way in which individuals and, above all, groups and institutions succeed in excluding potential issues from the political process. Decisions are choices consciously and intentionally made by indi-

viduals between alternatives, whereas the bias of the system can be mobilized, created and reinforced in ways that are neither consciously chosen nor the intended result of particular individuals' choices" (p. 21). Lukes is right on this point, as he is also right in noting that we recognized it in "Two Faces of Power" but unwisely retreated from it in *Power and Poverty* in reaction to pluralists' criticism.

Lukes also taxes us for embracing the pluralists' assumption that power is exercised only in situations of conflict. He observes, again correctly, that A may exercise power over B by "influencing, shaping or determining" B's very wants, such as, thought control or brainwashing. Far from ignoring or denying this point, Bachrach and I explicitly took it into account, particularly in our discussion of authority: ". . . just as authority can be transformed into power [we said], so can the reverse obtain. 'Brainwashing' after the manner of George Orwell's 'Big Brother' . . . is a gruesome case in point" (*Power and Poverty*, p. 35).

Lukes's third criticism of the two-dimensional view is closely linked to the second, namely, our alleged failure to recognize the possibility that absence of grievances can indicate, not genuine consensus but existence of a false or manipulated consensus. This condition could occur, of course, in circumstances in which B reacts to A on the basis of subjective preferences or interests that differ significantly from B's objective or "real" interests. It is debatable whether Bachrach and I have ignored this salient point (see, for example, our reply to Richard Merelman in the *American Political Science Review* [December 1968]). We agree with Lukes, in any case, that "men's wants may themselves be a product of a system which works against their interests" (p. 34)—as we also agree that there are imposing but not insuperable conceptual and empirical obstacles against determining the character and degree of divergence between a person's or group's subjective and objective preferences.

Lukes's plea for a three-dimensional view of power will not promptly be heeded by all who study the phenomenon. Those who resist, however, will have a difficult time rebutting his propositions, the more so because of the rigor with which they are stated and the trenchancy of the supporting discussion. Slim though it is, Lukes's essay is an important contribution. One way or another, all of us who subsequently address the problem of power must take his words into serious account.

Power: The Inner Experience. By David C. McClelland. New York: Halsted Press, 1975. Pp. xiv+427. $15.95.

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In the social sciences, the term "power" traditionally implies *domination:* one person or group exercises power *over* others. In this new book, David McClelland has begun doing with "power" what he has earlier done with

"achievement." In the process, he explodes the commonsense notion of the concept, reassembling the pieces along several dimensions and examining each from several points of view—ranging from individual psychology to international affairs. It is, to say the least, an ambitious book. It is also, appropriately, a very powerful one.

As in his earlier research, McClelland is less concerned with power per se than with the *need* for power, which he defines as "a thought about someone *having impact*" (p. 7), and "n Power" is sure to join "n Achievement" on psychology exams. The need for power is experienced and manifested in many ways including domination, but also through service, inspiration, advice, and countless other ways of "having impact."

Most of the book is an elaboration of McClelland's fundamental paradigm of power, constructed from two dimensions—the "source" and the "object" of power—each having the same two categories: "self" and "other." The resultant types represent, for McClelland, four stages in personal development and correspond to common psychoanalytic stages.

In Stage I, the source of power is other (e.g., God, mother, leader) and the object is self. Here, the individual gains strength from an external power source: for example, the child at its mother's breast.

In Stage II, both the source and object of power are self, as the individual gains self-control and autonomy.

In Stage III, the self remains the source of power, while other becomes the object. The individual begins to have a significant impact on other people.

Finally, in Stage IV, both the source and object of power are other, with the individual becoming a vehicle through which power operates. A "greater power," whether secular or sacred, operates in the world through a particular individual. Here we find the corporate managers, the scientists, and the messiahs.

In introducing the paradigm, McClelland discusses corresponding stages of child development, the kinds of adult actions corresponding to each, typical adult occupations reflecting each type of power, and common pathologies. Then he branches out, devoting the rest of the book to the application and elaboration of the paradigm in a variety of contexts.

Subsequent chapters examine power in relation to maturity, female power roles, national character studies of India and Mexico, modes of leadership in corporations, national empire building, and international patterns of war and peace. He addresses the fascinating Stage IV type, in part, through in-depth analyses of Baba Ram Dass (*né* Richard Alpert), Black Elk, and the Book of Revelation. In the latter discussion of "visions of power," McClelland brings together the openness of a Castenada with the tough mindedness of a clinical psychologist.

McClelland has greatly and usefully broadened the concept of power in ways of value for sociologists. He notes the American suspicion of power and those who seek it, and he shows that power has dimensions and nuances quite apart from its Watergate manifestations. It is possible to exercise power without domination. Power can be generated by giving

it away in some contexts. And "helping," on the other hand, is often a form of domination. These are but a few of the many insights into power that McClelland's broad-ranging analysis points to.

The main difficulty readers—especially sociologists—may have with this book lies in the multistudy collection of data analyzed. McClelland references countless (I lost count) studies of his own and of others in exploring specific aspects of the power paradigm. Most are based on small, nonrepresentative samples, requiring heavy reliance on tests of statistical significance. They are all brought into play in creating a weight of evidence in support of the paradigm, and it is sometimes difficult to keep track of the many studies as well as the continuity of the analysis.

All this notwithstanding, the book is an excellent and instructive methodological exercise. McClelland exemplifies the logic of social scientific inquiry at its finest and most creative. Confronting a theoretical proposition, he asks "what we ought to observe where" if the proposition is correct. He is not stuck on a particular research technique nor on a particular type of data. Social psychological patterns are manifested in many forms, and McClelland is willing to consider any of them.

McClelland is an insightfully reflexive methodologist, and his many methodological discussions reflect the native good sense we find in a Stouffer or a Lazarsfeld. This is an important and powerful book in many ways and well worth reading.